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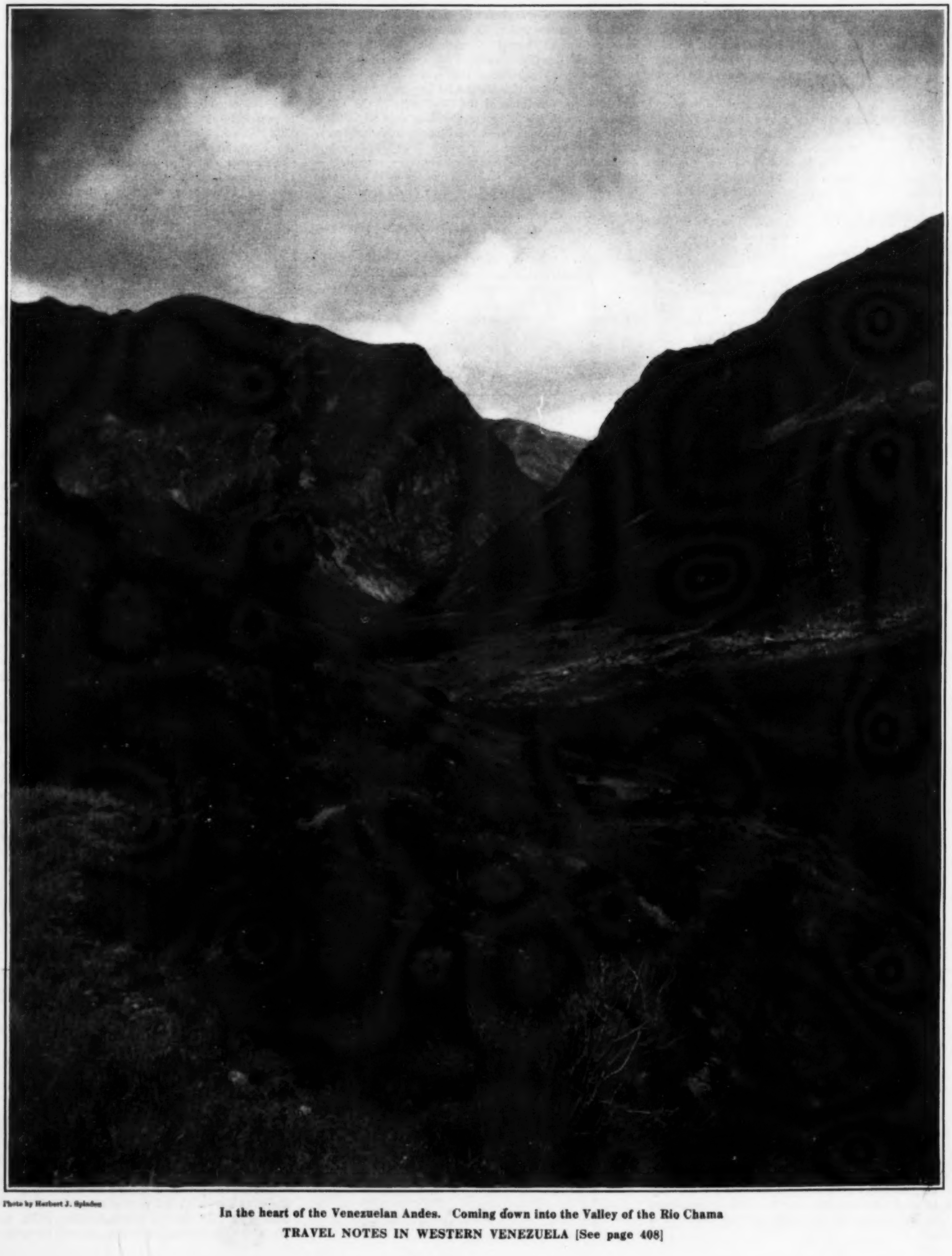


Photo by Herbert J. Spinden

In the heart of the Venezuelan Andes. Coming down into the Valley of the Rio Chama

TRAVEL NOTES IN WESTERN VENEZUELA [See page 408]

Chronology of the Egyptian Pyramids*

And Problems Suggested by the Details of Their Construction

By F. J. B. Cordiero

These monuments of remote antiquity have engaged the curiosity of man from time immemorial. A host of travelers, Greek, Roman, medieval Arabian, and modern, have examined them, measured them, pondered over them and left us their opinions. When were they built, what was their purpose, and what hidden meaning do they contain? We know very little more of these things than Herodotus did. He recognized, as we to-day know, that they were the tombs of great personages—ancient rulers—but beyond that we cannot go with any certainty.

The bibliography since 1799, the year that the scholars attached to Napoleon's army of invasion founded their Egyptian Institute, is enormous. Among many of these writers the pyramids have exercised a peculiar influence, in that it has led them to form scientific or religious theories which they sought to justify through them, or has led them to attempt to fit preconceived theories to what they found there. This influence has not only left its mark upon the genus "Crank," as was to be expected, but has also attacked some of the most conservative men of science.

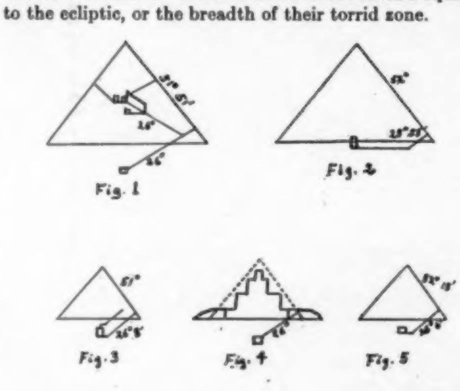
In 1840, an individual known as Mr. John Taylor, of Gower St., London, founded a religious sect known as the "Pyramidists," their religion being directly revealed in the Great Pyramid. There is nothing very remarkable in this, but it is remarkable that twenty years afterwards Professor Piazzi Smyth in Scotland, and the Abbé Moigno in France, became the chief prophets of this new religion. Now both of these gentlemen were conservative scientific men, logical by nature, of excellent general judgment, and trained in the strict methods of the mathematico-physical sciences. Professor Smyth was for many years Astronomer-Royal of Scotland, and a most competent and distinguished astronomer. The Abbé Moigno was the scientific associate of Leverrier and well known to mathematicians by his work. According to these gentlemen, the exact measures of all the constants of the solar system are contained in the Great Pyramid. A more exact value of the distance of the sun from the earth can be found in the Great Pyramid than by the most refined observations and calculations, by reason of the fact that the builders put it there by "divine inspiration." The length of the polar axis, the precessional period, the sidereal day, the year, and in fact all measures are accurately recorded there.

Professor R. A. Proctor took the pains to demolish these contentions one by one, which he did most successfully and with evident delight. There have been few clearer minds than his, and it would seem unlikely that one who so readily detected a flaw in another's argument would permit an error to enter into his own. And yet it would seem that this fatal pyramid influence did not leave Professor Proctor unscathed. After saying that "It is almost impossible to mark any limits to what may be regarded as evidence of design by a coincidence-hunter," and quoting from De Morgan's "Budget of Paradoxes," he goes on to *prove* that it was the design of the builders of the Great Pyramid to place the center of its base exactly on the thirtieth parallel of latitude. True, it is not there, but about one and one-half miles to the south of this parallel. But he adds, the reason is that they knew nothing of atmospheric refraction. Further, according to Professor Proctor, this error shows that these ancient astronomers must have used the northern stars in determining their latitude, and not the sun.

Now there are some thirty-eight other pyramids within twenty miles of the Great Pyramid on the left bank of the Nile, varying in latitude from 29° 17′ to 30° 4′. In the immediate vicinity of the Great Pyramid and *near* the Nile, there is no spot where it would be possible to raise such a structure exactly on the thirtieth parallel. For the Great Pyramid is on the extreme northern verge of the old lime stone reef which was washed by the ancient Mediterranean prior to the formation of the Nile delta. If the builders had attempted placing it farther north, and anywhere near the river, they would have to have dug through an interminable amount of Nile silt before finding bed-rock.

It is evident that the first consideration was to find a suitable rock surface which could be planed down to form a platform. The particular latitude of such a platform did not matter, as is clear from the other pyramids.

The orientation of these pyramids, which all have their bases very exactly in the cardinal directions, could only have been determined by astronomical observations, and the builders were undoubtedly astronomers of a high order. We may grant much, but it seems to be unnecessary to grant more than is called for. They understood goniometry perfectly and probably could measure their angles within one minute. They could determine latitudes quite accurately, but that they knew that the earth was a globe and what its diameter is, as Professor Proctor maintains, seems doubtful. As they knew nothing of theoretical mechanics, they could not have known that the earth is compressed, and they could not have calculated the return of comets, as Diodorus Siculus credits them with having done. They probably knew nothing of the Precession of the Equinoxes, although it is barely possible that they might have detected this through careful and long-recorded observations, just as thousands of years later Hipparachus detected it. They undoubtedly knew very accurately—within one minute—the inclination of the earth's equator to the ecliptic, or the breadth of their torrid zone.



Pyramids around Gizeh meridional sections: Fig. 1, Khufu (Cheops); Fig. 2, Cephren; Fig. 3, Menkaura

It is astonishing that all coincidences, analogies and theories have heretofore been taken solely from linear measures of the Great Pyramid, while no attention has been paid to the angles. In 1638, Professor Greaves, then professor of astronomy at Oxford and Astronomer-Royal of England, visited the Great Pyramid and made some careful measurements. He gave the inclination of the entrance passage as 26°. Angles did not enter into Professor Piazzi Smyth's scheme and all that he has to say about them is, "The angle of the rise of the Pyramid's flanks and the angle of descent and ascent of its passages are both peculiar angles, characteristic of the Great Pyramid." Professor Smyth gives the inclination of the faces of the pyramid as 51° 51′, and the inclination of the descending and ascending passages as exactly alike, and 26°. These angles are not only characteristic of the Great Pyramid, as he says, but are characteristic of all the other pyramids. The only suggestion that Professor Smyth has to make in regard to this particular angle is that, provided the builders divided their circle into 1,000 parts, then "The angles 26° and 52° would be, after a fashion, commensurable parts of a whole circle." The trouble is, they are not. Since at various times, before or after, there was probably a connection between the Egyptian and Babylonian astronomers, it seems likely that they divided their circle into 360 parts, but we have no knowledge on this point.

A writer in the Encyclopedia Britannica has suggested that this particular angle was chosen because its tangent is one-half, *i. e.*, the rise is exactly one-half of the advance. But the angle whose tangent is one-half is 26° 34′, and this is an angle which the pyramid builders could not possibly have confounded with their own peculiar angle.

The most reliable angle measurements that we have of the first nine pyramids around Gizeh, are as follows:

	Slope		Passages	
	°	′	°	′
1 Khufu (Cheops)	51	51	26	
2 Cephren	52		25	55
3 Menkaura	51		26	2
4	—		26	
5	52	15	26	
6	—		26	
7	52	10	26	
8	52	10	26	
9	52	10	26	

These angles, while in each individual case only approximate, give us as average values 26° and its double 52°. The sixth, seventh, eighth and ninth pyramids have crumbled down into ruinous heaps, and are probably older than the others.

Now there can be no doubt as to the reality of this pyramid angle. It is a particular and carefully selected angle, and it "sticks out" everywhere. The question naturally arises—why this particular angle, when there were so many other angles that might just as well and just as easily have been chosen? Before answering this question, let us be sure that we are not overborne by the "Pyramid Influence" which can carry away not only the plain "Crank," but an Astronomer-Royal and a genius like Professor Proctor. Let us not merely hunt for coincidences and then build up theories upon them. There are coincidences, and coincidences, and it is the province of the theory of probabilities to sift such cases.

In the first place the angles are everywhere the same, but these are not coincidences. The agreement was planned; it was not accidental. Was there any angle which would be likely to impress itself upon them and lead them to adopt it for their pyramids, as their sacred angle? Yes. There was then, and there still is, one angle and only one angle in all nature which presents itself unmistakably to mankind. This angle is the inclination of the equator to the ecliptic, or the breadth of the tropical zone. The ancient Egyptian astronomers certainly knew this angle to a minute. We need not suppose that they had any idea of the revolution of the earth about the sun, or the real reason for this seasonal wandering of the sun, for they probably had not; they simply knew the angle because they had measured it, and quite accurately at that. If two men, wholly independently of each other, are to come to me and utter some word, the chances that those words shall be the same are infinitesimal. But if they should utter the same word, out of all the possible words they might have chosen, then it approaches a certainty that there was collusion. Such a coincidence savors of conscious mental effort.

But, it may be said, the tropical angle—the angle between the equator and ecliptic—is only 23° 27′. That is the present angle, but was it the angle at the time the pyramids were built? Decidedly not. The angle at *that* time was in all probability around 26°.

Less than 400 years ago, the general belief was that the earth did not move at all; it was supposed to be absolutely stationary. Finally the evidence became overwhelming that it must have at least two motions—a rotation and a revolution about the sun. For this view to become accepted it was necessary for the scientific men of the day to be *willing* to examine and weigh the evidence. This was for a long time an impossibility. In the next place it was necessary that they should be *able* to *understand* the nature of the question, but though the mentality required here is very moderate, there were further difficulties. Two hundred years ago, Bradley rediscovered the Precession of the Equinoxes by observation just as Hipparachus had originally discovered it, 120 B. C., and Newton gave the reason for it. This was ignored for a long time, but finally accepted, rather upon authority than from any distinct understanding of the cause. There is still another motion of the earth which theory shows must exist, and which does exist. This is due to the elasticity of the earth and consists of a slow pendulation of the axis of the earth through the pole of the ecliptic. The earth's axis describes a small precessional circle about the pole of the ecliptic, and if the earth were *absolutely* rigid, these circles would be closed curves and the inclination would remain forever practically unchanged. But the earth is not absolutely rigid, with the result that the precessional circlings are not closed curves, but spiral gradually inward. In a very long time, therefore, the inclination of the earth's axis undergoes very considerable changes. The reader who wishes to inquire further into these motions is referred to Routh's "Rigid Dynamics," "The Gyroscope" (Spon and Chamberlain), and an article in *Popular Astronomy*," August, 1915. Our data as to various inclinations of the earth's axis in the past are necessarily scant and not very reliable. Perhaps the best value we have is that of Erostosthenes (236 B. C.) who found it between 23° 52′ and 23° 51′. Hipparachus (120 B. C.) is said to have found it 23° 51′.

Taking this value, it will be necessary to go back to about 10,000 B. C. before the inclination is 26°. Using Ptolemy's (130 A. D.) determination, which is in all probability erroneous, the date would be 6370 B. C.

**Popular Astronomy.*

The estimates of the time of the pyramids vary widely—all the way from 3000 B. C. to 13,000 B. C., and even more. The simple fact is, we do not know when they were built.

If, however, the angle 26° represents the natural angle at the time they were built, then we have some definite land marks in our chronology. In that case we have a permanent record of the inclination of the earth's axis at some time in remote antiquity, and knowing the rate we might find the time, and vice versa.

It is, of course, possible that the angle 26° marks only the era when pyramids first began to be built, and that afterwards this angle was slavishly and religiously copied. In this case, the angle would lose its value as a chronological factor. But it seems more probable that the builders who were able to determine the angle were also able to revise, and did revise it, from time to time. Possibly the slightly greater angle in the older pyramids—seventh to ninth—points to a greater age by some six to seven hundred years.

The pyramid builders selected a peculiar angle—in the neighborhood of 26°, and its double 52°—which they put into all their structures. They were astronomers and geometers of a high order, as their mathematical structures prove. That they should have selected as their standard pyramid angle the breadth of their then tropical zone, seems natural, as it was and still is, the only definite angle presenting itself in nature. It is practically certain that the inclination of the earth's axis had this value about 10,000 B. C. There is a strong presumption, therefore, that the pyramid angle represents the great angle of nature. With less certainty, we can infer that the pyramids were built some time between 11,000 B. C. and 6000 B. C.

A New System of Weather Prediction

By Which Conditions May Be Predicted Months in Advance

By René Paresce

In spite of the enormous amount of meteorologic data accumulated in the last few decades, weather prediction is still to be regarded as an infant science, though it is the continual effort of modern scientists to substitute general laws for empiric methods wherever possible. Considering the difficulties of the problem, however, the results obtained may be considered remarkable. For example, the records of the Meteorologic Bureau at London show that out of 100 weather predictions fifty-six are verified, and thirty-one are approximately correct.

It is hoped, however, that in the not distant future far better results may be obtained, and the work already done by an Italian "weather sharp," Mr. F. Vercelli, enhances these hopes. Mr. Vercelli has very recently published a work, entitled "Periodic Oscillations and Prediction of Atmospheric Pressure," under the auspices of the Royal Observatory of Brera at Milan. We quote from a review of this book in *La Nature:*

It has long been recognized that a diagram of atmospheric pressure, obtained by any type of registering barometer, presents a certain regularity—in spite of its apparently irregular and spasmodic action—which becomes visible when a sufficiently long period of time is taken under observation.

Atmospheric pressure oscillates between certain limits given by the following law: The interval within which barometric pressure oscillates increases with the latitude, and, up to 60 degrees, it is very nearly proportional to the fourth power of the latitude itself.

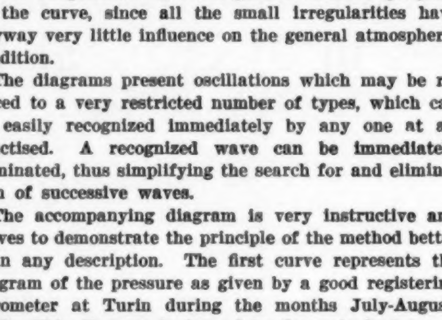
Up to the present time scientists have contented themselves with verifying the existence of these oscillations of pressure and determining their elements: amplitude and duration. In this way there have been discovered the period of 1, 2, 4, 6, 8, 9, 16, 32 days; oscillations masked by an *ensemble* of phenomena impossible to forsee in advance because of their extreme degree of confusion and intricacy. This discovery, though of importance, could be utilized only very imperfectly to determine the atmospheric state of a period of time comprised within given limits. This is why meteorologists have been forced to confine themselves to predicting the weather from day to day, without foretelling atmospheric conditions for a month or even a week in advance.

But are these variations of the barometric diagrams really irregularities? May they not be rather a consequence, complicated, it is true, of a certain number of very simple and regular phenomena? May not the undulations of pressure be the resultant of a certain number of elementary oscillations very regular in occurrence? It is precisely this point which has fixed the attention of Prof. Vercelli, who has just published the results of his investigations of atmospheric pressure. These are surprising, and it may safely be said that henceforth new vistas are open to meteorology. Mr. Vercelli has remarked, in fact, that all the diagrams he has studied comprise a small number of regular oscillations, of very definite and constant periods, and of an amplitude which decreases with the time.

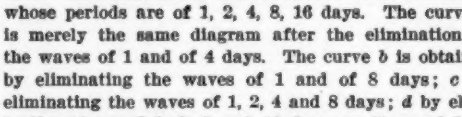
It is merely a question, therefore, of solving the following purely mathematical problem: Given a curve, composed of an indefinite number n of periodic waves, to determine the periods of the component waves. This problem being resolved, it is easy to see in what manner it can be applied to a barometric diagram. A certain wave being determined, one can eliminate it from the diagram by means of an appropriate graphic method. The diagram resulting from this elimination will present a simpler aspect which will permit us to recognize another component wave which can be eliminated in its turn. The problem will evidently be very simple if the waves are not neutralized, which is not the case with atmospheric oscillations. It is necessary in this case to limit ourselves to a certain degree of approximation and to endeavor to obtain by synthesis of the component waves the general mode of progress of the curve, since all the small irregularities have anyway very little influence on the general atmospheric condition.

The diagrams present oscillations which may be reduced to a very restricted number of types, which can be easily recognized immediately by any one at all practised. A recognized wave can be immediately eliminated, thus simplifying the search for and elimination of successive waves.

The accompanying diagram is very instructive and serves to demonstrate the principle of the method better than any description. The first curve represents the diagram of the pressure as given by a good registering barometer at Turin during the months July-August, 1913. This diagram is formed by five principal waves whose periods are of 1, 2, 4, 8, 16 days. The curve a is merely the same diagram after the elimination of the waves of 1 and of 4 days. The curve b is obtained by eliminating the waves of 1 and of 8 days; c by eliminating the waves of 1, 2, 4 and 8 days; d by eliminating those of 1, 2, 8 and 16 days; e, those of 1, 2, 4 and 16 days. The regularity of the residual waves is already evident; the five types of waves could not be more regular.

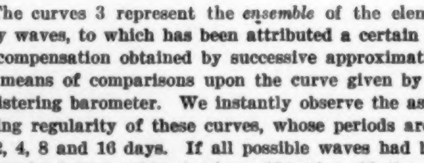


1. Observed curve



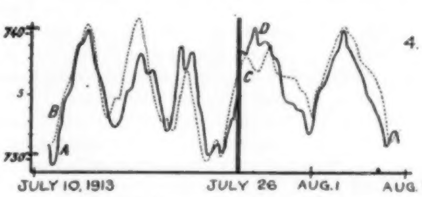
2. A derived curve

The curves 3 represent the *ensemble* of the elementary waves, to which has been attributed a certain law of compensation obtained by successive approximations by means of comparisons upon the curve given by the registering barometer. We instantly observe the astonishing regularity of these curves, whose periods are of 1, 2, 4, 8 and 16 days. If all possible waves had been determined and taken into consideration, if the law of compensation had been precisely established and applied, the addition of these waves, or the synthesis of these curves, ought to exactly reproduce the diagram.



3. Composite waves and synthesis of barograms

The portion at the left of the curve 4, drawn with a continuous line, represents the portion of the diagram of atmospheric pressure between the 10th and the 26th of July, 1913. The dotted curve is the synthesis of the elementary waves obtained by the method indicated, and represented in Fig. 3. One easily sees that the synthetic curve follows the form of the given curve, reproducing all its peculiarities. The mean distance between the curves A and B is on the average one millimeter of the mercury. What does this distance signify? We have already said: to have a perfect coincidence of the two curves it would be necessary to take account of other waves of shorter period and establish more exactly the law of compensation. But since the little irregularities have only a very feeble influence on the special problem of foretelling the weather, we may retain without retouching the curves admitted in Fig. 3. Consequently it appears obvious that the problem of weather prediction is solved.



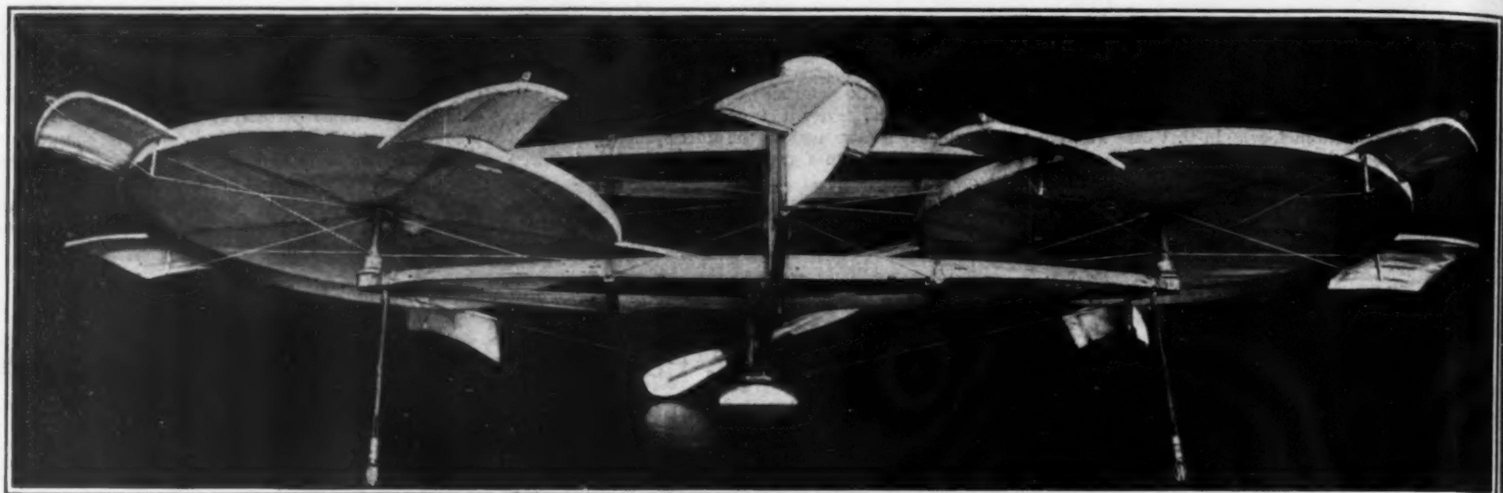
4. Pressure diagram and synthetic curve

If the elementary curves have so simple a mode of progress, if the period of the component waves has been recognized as constant, we have only to prolong each curve, taking account of the compensation. The synthesis of this new portion of the curve will then give the curve of the pressure during an interval of time following that which has already elapsed.

The dotted curve C, at the right of Fig. 4, is the curve foretold for the interval of time between July 26th and August 7th, 1913. On August 7th the record given by the registering barometer was taken; it is represented by the curve D in the same figure. Thus the prediction of the atmospheric pressure was made for a period of thirteen days with an absolutely unhoped for precision.

But more often the difficulty of the problem is much greater; waves of a period of 32 days intervene, at the same time as petty diurnal and semi-diurnal variations. New waves of the period of 4 and 8 days appear, then disappear to appear anew with another law of compensation. But although all this complicates the problem, its solution is always possible with more or less approximate accuracy, since all the waves studied have been recognized as belonging to a few very simple types. And these types are not characteristic of a given locality, but present themselves indifferently in all places and at all times. Mr. Vercelli thus calculates the curve of pressure at Biarritz, at Palermo, and at Alexandria (Egypt), and shows the identity of these types of component waves.

It is obvious from the foregoing what a new path is opened to meteorology by the results obtained by Mr. Vercelli: a more and more exact determination of types of waves and causes of undulatory phenomena of atmospheric pressure. This last point is above all of the greatest importance: the cause of the phenomena once discovered it will be easy to obtain a general law of compensation of waves, which must inevitably depend upon all the physical phenomena which are manifested in the atmosphere. It will be possible, moreover, to foresee transient waves of variable amplitude; in other words, it will be possible to understand the manner in which the component waves are produced. The essential phenomenon is discovered, the irreducible irregularity of variations of pressure is demonstrated to be purely apparent. When the cause is discovered meteorology also will possess scientific laws. The prediction of the weather for a period of several months, or for an entire year, will not merely cease to be a Utopian dream, but on the contrary will be realized without the aid of chance. Meteorology, an empirical science, will thus become a rational science.



Rotating disk model, viewed from below, showing plane surfaces with dependent rims, alaries or lifts, and universal rudder

Experiments with the Rotating Disk and Peripheral Alaries in Mechanical Flight

By Justin Kay Toles

THESE experiments were conducted with a view to developing a flying machine which could lift its own weight and a given load vertically, which, when in the air could hover, and which in case of accident could be brought to earth with reasonable safety.

Starting with the central idea of the revolving disk for inherent stability, and peripheral alary lifts as a means to overcome dead load, many forms of both were evolved before those shown were finally selected.

Gyroscopic speed is not necessary to maintain equilibrium in a rotating disk. The quickest way to refute this idea is for any one to take a barrel hoop and stretch over it a piece of ordinary sheeting. Holding this disk-plane in front of the body so that it is horizontal with the earth's surface, and projecting it forward with rim dependent, so that the disk makes about one revolution to, say, about one hundred feet of forward travel, will give one a fair idea of the *inherent* stability of the *dependent rim disk*.

This *inherent* stabilizing quality of the rotating disk is but one important step in the development of the central idea. The peripheral alary lift is the other.

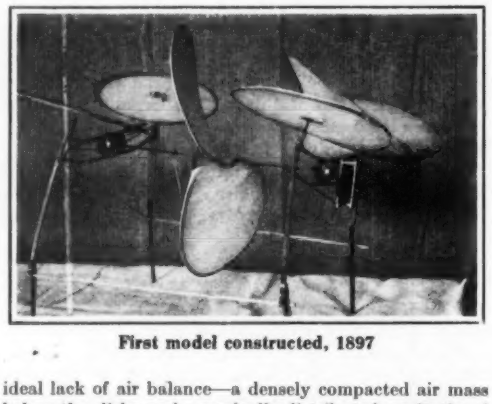
The embodiment of these two principles give to a purely mechanical construction the inherent principles of bird flight, *i. e.*, birds receive their power to equilibrize, so to speak, from the neuro-sense organism controlling balance, same as man in walking. This is imperfectly possible through the lever control of the present mode of flight.

Rotation absorbs all external influences attempting to destroy balance; while in the lifting alaries *direction* is given and controlled; that is, direction of balance. It is one thing to know that one is going to fall; it is quite another to control the direction and readjust one's balance. These are the *inherent* functions of the combination disks and alaries (lifts). The latter are similar to a swimmer's arms in promoting lateral equilibrium as well as functionally displacing his weight.

These lifts are in no sense helicoptres; that is, they have no screw action.

To go back to the disk. The *dependent rim* is functionally important. If the barrel hoop be turned over so that the plane-surface becomes inferior (rim uppermost) and projected forward, its flight is materially shortened; air striking the projecting rim is forced over it in the form of built-up vortices which occupy the first two-thirds of the peripheral area of the disk's entire inferior surface, with the greater density toward the periphery. Here this density exerts pressure. This pressure is rotational—at all points alike, therefore peripherally uniform. The unequal pressure between the inferior and the superior surfaces either shortens or lengthens the flight, depending entirely on the disk being *projected* right or wrong side uppermost. When right side uppermost the denser volume of air being underneath gives the disk greater buoyancy and prolongs the flight, and what is vastly more important, imparts to it by reason of the rapid accumulation of air vortices behind the dependent rim, greater stability. Hence equilibrium in spite of relatively slow rotation. In absence of the dependent rim this phenomenon is not evident. The alary lifts are in themselves perfect stabilizers. Also, they accentuate buoyancy by forcing below and under the disks an added density of air, until there is actually built up thereunder a well made air road over which the disks travel when projected in a forward direction.

Another phenomenon is apparent. Immediately above the disks there is created a cavitation with no perceptible air movement for a considerable (conical) distance above them. This phenomenon provides an

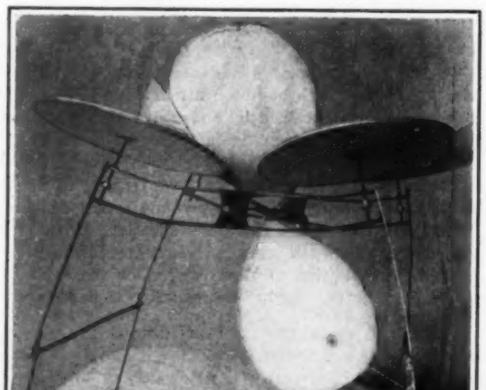


First model constructed, 1897

ideal lack of air balance—a densely compacted air mass below the disks and a markedly distributed cavitational air void above.

The larger set of the accompanying photographs are of a scale model evolved from a series of experiments conducted in 1898 and was made in the latter year.

In the fall of 1900 a two-disk test model, one-half full size, was constructed on the lines shown in the photo-



First model, 1897

graphs of this (one inch to one foot) scale model. Each disk measured six feet in diameter, having dependent rims 4 inches deep; and each have five alary lifts 11 inches wides and 16 inches long. The model was flexibly mounted on a rotating arm having a radius of 50 feet.

When the disks were rotated 200 r.p.m. they developed in *dead air* a vertical pull or lift of 21 pounds per horse-power; and 36½ pounds per horse-power when the balanced arm was rotated 40 r.p.m. Power was transmitted through a flexible coupling so that the disks would have to adjust themselves to their own centers of balance, or equilibrium; which was always maintained without any perceptible *drag* on the disks while the arm was being rotated by hand and without the aeroplane propeller operating—provided these rotations were not in linear feet greater than the peripheral speed of the disks. The maximum speed of the rotating arm when driven by the aeroplane propeller was 38.27 r.p.m.

Lateral, forward or currents of air from the rear had no visible influence on the disks provided the air was not stronger in feet per minute than the peripheral speed of the disks. It is interesting to note that the revolutions of the beam were not slackened nor the disks deflected excepting vertically and on an even keel, by any sudden blasts of air within the limits of the combined peripheral speeds.

ADDENDA

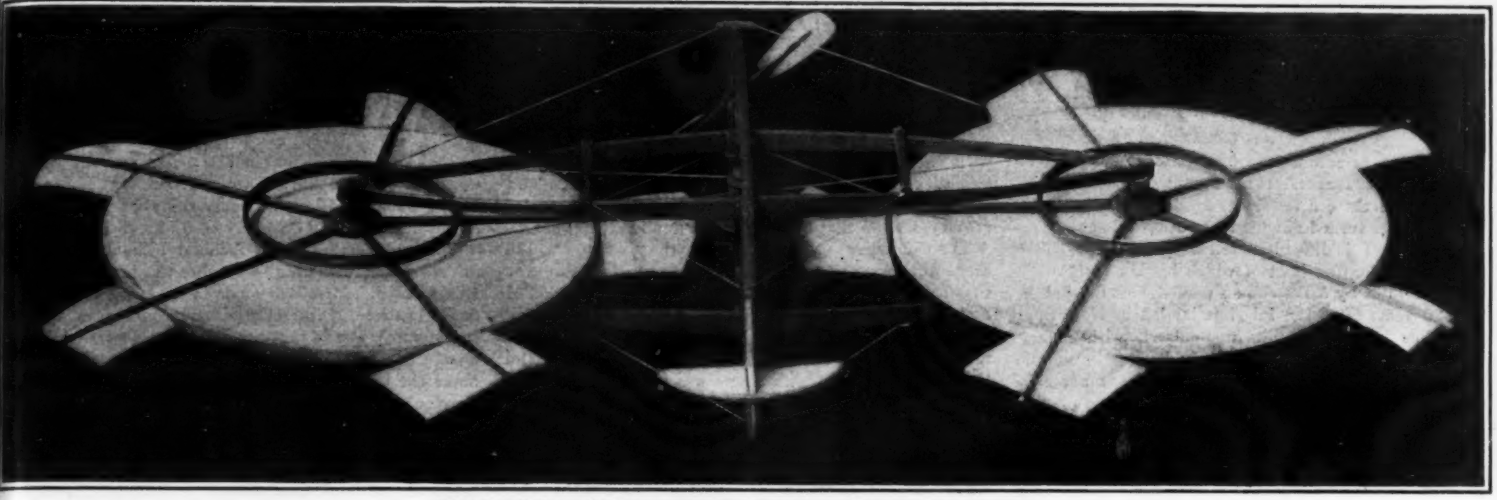
In 1898 in my "journal of records," I made the following observations:

"It has always been apparent to me that investigators of mechanical flight who have used the bird as their model in constructing experimental machines, are contending against this fact: a bird, that it may maintain its equilibrium in flight, is endowed with a correlative neuro-sense organism, for which it is obviously impossible to supply an adequate substitute in any man-constructed machine. Destroy this sense of correlation between the body and brain of the bird and it cannot perform the function of flight, any more than a man can maintain his equilibrium or power to walk after the channels of sense transmission between his extremities and his brain have been destroyed.

"The requisites of a successful flying machine, are *first*, its power to maintain, under all circumstances, a perfect equilibrium; and *second*, the power to rise from the ground, in a calm, with not only its own weight, the weight of its power apparatus and operator, but to be of any practical value, at least 50 pounds per horse-power additional burden. That is to say: It must be able to rise into the air by its own internal energy, and independent of launching ways, or extraordinary wind velocity.

"I have in my experiments employed the essential element of the principle involved in the maintenance of balance in a spinning top. Since this principle is by no means confined to any one thing, but is demonstrable in anything having the power to be revolved on a free center, it is taking it but one step further to apply it to the subject in hand."

I take, for illustration, a disk of paper, or cloth, limp or stiff, it matters little, and revolve it; so long as it is revolved at a high enough velocity it is self-balancing and largely self-sustaining. I give it a forward impetus and let it continue to revolve; if the conditions of velocity remain unaltered, it will remain balanced while gliding through the air at whatever angle of incident I will it to leave my land. I take the same disk and push it forward with equal impetus without revolving it and it has absolutely no power of equilibrium. *Hence*



Rotating disk model in perspective from rear, when used as a tractor

the function of revolution compels the disk to assimilate balance. Nor is this phenomenon of equilibrium destroyed, or in the least impaired by the impulse of forward movement given it. On the contrary, the combination of these two forces assist each other in their individual functions.

If, in throwing the disk into the air I tilt it so that it is lowest at its right or left side it will describe a corresponding curve; *at once proving the principle by which a machine may be steered with no other rudder than the reciprocal action of its own disks.*

During the month of July (1897) I constructed a model on this principle shown on the preceding page.

The disks (four in all) were made with wooden rims, over which was snugly drawn Japanese paper, with wooden centers large enough only to hold the axles, or disk shafts in place.

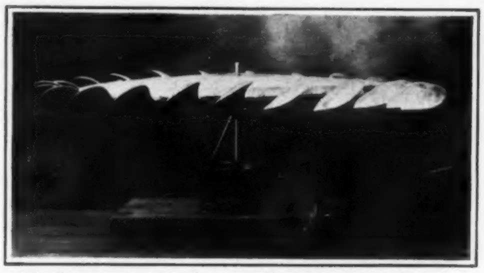
The front disks had each 36 and the rear disks 81 square inches; in all 234 square inches sustaining surface.

The screw propeller measured 11 inches diameter, five inches lateral dimension, with a measured thrust of three inches. This screw was made of bamboo covered with Japanese paper, and fixed to the main driving shaft. Back and forward of the propeller and fixed to the same shaft were two compound aluminum pulleys fixed to the shafts of the four disks.

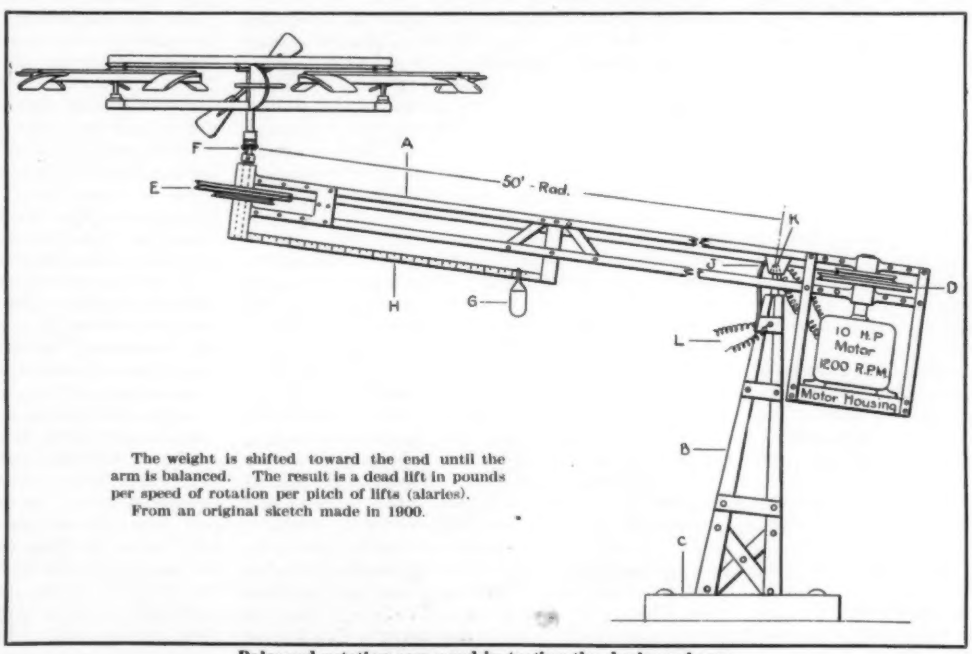
The pulleys were in such relation to each other that the disks were made to revolve with about three times the velocity of the propeller. The frame of the machine was made partly of bamboo and partly of Spanish cedar. The power was derived from the spring of an alarm clock set in an aluminum casing.

Weight of machine when completed, 13¾ ounces.

At trials given this machine, it shoved forward on its rear supports, or legs, about sixteen inches, at the same time rising slightly until the front legs were some three inches off the floor. There was in repeated trials an apparent effort of the "hind" legs to lift free of the floor, which they would do for intervals of a few inches. The power would be rapidly exhausted and the distance traversed would never be more than 30 feet. But at all times there showed a disposition toward perfect stability.



Earliest form of peripheral alary, replica of honey bee's wings, constructed 1908



The weight is shifted toward the end until the arm is balanced. The result is a dead lift in pounds per speed of rotation per pitch of lifts (alaries). From an original sketch made in 1900.

Balanced rotating arm used in testing the devices shown

Experiments on Ascaris Infection in Hong Kong

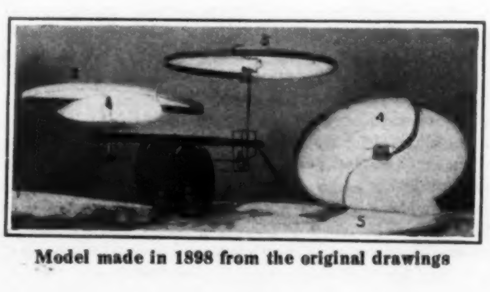
An important paper by Capt. F. H. Stewart, Indian Medical Service, appeared in the *British Medical Journal* for July 1st, giving the life-history of *Ascaris lumbricoides*, which is extremely common both in man and the pig at Hong Kong, where the author is stationed with the 74th Punjabis. In this preliminary communication he showed that the parasite presents an alternation of hosts. Thus, when ripe eggs reach the alimentary canal of the rat or mouse the larvae are liberated, and six days after infection they are found in the blood-vessels of the lungs and liver, and the host is seriously ill with pneumonia. They next pass from the blood-vessels into the air-vesicles of the lung, causing hemorrhage in them. On the tenth day they occur only in the vesicles and in the bronchi. If the disease does not prove fatal, the host recovers on the eleventh or twelfth day, while on the sixteenth day it is free from parasites. The affected animals could readily contaminate by the nose or mouth the food of man or the dust and earth of his surroundings.

Capt. Stewart has continued his experiments since the foregoing date both with *A. lumbricoides* and *A. suilla*, and finds that the larvae appear in the bronchi, trachea, and mouth of the rat and mouse on the night of the seventh day and during the eighth day after infection by the mouth, and he believes that they pass by means of the saliva on to the food which is being nibbled by the rodents. It is possible that one attack of ascariasis in rats renders them immune against subsequent attacks, but further confirmation is necessary. He found that the larvae survived longest (24 hours) in blood on moist bread. In water, normal salt-solution, and in mouse's blood they survived three hours.

Out of five experiments to test the infection of pigs from the foregoing rodents, three gave positive results, two negative. In estimating the value of the negative experiments the very high mortality among the parasites employed under somewhat unnatural conditions must be kept in mind. Capt. Stewart endeavored to obtain an estimate of this mortality by comparing the number of ripe eggs given to a mouse with the number of larvae found in the lung. An average dose contained about five thousand eggs, whilst the number of larvae found in the lungs did not exceed fifty. The transfer from the rodent to the pig is probably the most vulnerable part of the life-cycle, since the larva is a very delicate organism. The author also carried out control experiments with the pig.

Lastly, Capt. Stewart carried out some experiments which demonstrated that *A. marginata* of the dog has also its intermediate host in the mouse.

He concludes by stating that if ripe eggs of *A. lumbricoides* are swallowed by rats or mice they hatch. The larvae bore into the venules of the portal system or ascend the bile-duct. They are found in the dilated capillaries of the liver between the second and the fifth days. As their diameter is three times that of a blood-corpuscle in the mouse, they cannot pass through a normal capillary. The liver-cells in the neighborhood of the larvae undergo rapid degeneration, and the larvae are thus enabled to pass by the hepatic vein and vena cava to the heart, and by the pulmonary artery to the lungs, where they are filtered off at the entrance to the capillary field. Embolism of the arterioles takes place, and the larvae pass with the effused blood into the air-vesicles on the sixth day. They are found in the bronchi on the seventh day, and in the trachea and mouth on the eighth day, after infection. The larvae from the lungs of rodents can infect the pig, and it is probable that in Nature infection of both man and the pig takes place by food contaminated by rats and mice.—W. C. M. in *Nature*.



Model made in 1898 from the original drawings

The Freezing Point of Mercury

On page 232, of the issue of April 14th, the freezing point of mercury, as determined by the Bureau of Standards, was given as 638.873°C. Most of our readers will recognize that this was a typographical error. The correct figure is —38.873° C.

The Submarine Boat

Its Inception and Development*

By John P. Holland, Jr.

The history of submarines extends over a period of three centuries or longer. It is one long series of failures, with here and there a few meager successes, up to the time my father turned out his first successful submarine boat in 1877. It is difficult to tell who invented the first submarine. The first boat of which history makes any record was built by a Dutchman named Van Driebel, in 1640—20 years after the Pilgrims landed in New England. This boat was built in England, with money advanced by King James I. She had a rather unique ballasting system. There were a number of goatskin bags placed under the deck, between two large planks. These bags, when filled with water, caused the boat to sink. To cause the boat to rise the two planks were pressed together with a windlass arrangement, forcing the water out, which gave the boat reserve buoyancy. It is said that on one occasion King James was taken for a trip from London Bridge to a point about a mile down stream submerged in this boat. We do not know, however, what eventually became of her.

The first submarine that attained any measure of success was built by David Bushnell, in Connecticut, during the American Revolution. Money was supplied by the State of Maine. The submarine was tested on Long Island Sound. It was a small boat, built to accommodate only one man, and was called the *Turtle*, owing to her peculiar shape. Her ballast tank was beneath the floor, and she carried a detachable lead keel to give reserve buoyancy in case of trouble. The inlet valve was beneath the operator's foot, and the water was expelled from the ballast tank by a hand-pump. The boat was propelled by a screw propeller, turned by a hand-crank. Above the operator's head was a screw which was to be used for attaching a torpedo to the bottom of the boat attacked. When the torpedo was secured by this screw to the bottom of the boat the submarine made its escape. A time fuse attached to the torpedo would operate after the submarine had gotten clear. After testing the boat on Long Island Sound, Bushnell found that he could successfully blow up a ship in this manner.

Bushnell, not being very strong, prevailed upon Sergeant Lee, of the Continental Army, to attack the British ship-of-war *Eagle*, then at anchor off Governor's Island. He filled his ballast tank and sank to what we call the "awash" position and slowly proceeded in the direction of the *Eagle*. The current took him out of the way, but he finally managed to get beneath the ship. He then proceeded to drive the screw into the planking, but every time he attempted it the submarine would back away, because he had not sufficient reserve buoyancy. Had he remembered to cast loose his lead keel he would have overcome that difficulty. Breathing becoming difficult, he cast loose his torpedo and made his escape. A short time afterward the torpedo blew up, but it had drifted too far from the ship to do any harm.

Next we have Robert Fulton's submarine boat, built in 1801. He tried to obtain money in this country to further his ideas, but met with no success. He went to France, where a company was organized and financed by the French government. Agreement was made to the effect that for every English ship Fulton should destroy his company would receive a certain royalty, the amount of the royalty depending upon the size of the ship and the number of guns she carried. Fulton's boat was the same in principle as Bushnell's. The shape of the hull, however, was different. On the surface, instead of being propelled by a screw propeller, she had a collapsible mast and sail. The French government soon lost interest in the submarine, however, when Fulton did not go out to destroy the English fleet lying outside Toulon harbor. Becoming discouraged, Fulton went to England, but the English Admiralty, while they admitted that it was a good idea and might be useful to some nations, stated it would be absolutely of no use to the English navy. He finally came back to America, where he devoted his time and energy in the development of the steamboat. Fulton started work on another submarine, the *Mute*, but his death cut short the work, and it was never completed.

The first German submarine boat, on her trial trip in 1850, collapsed in the harbor of Kiel. It was not built strong enough to withstand the water pressure at the depth the pilot tried to navigate. However, he managed to get the conning tower open and to come to the surface alive. The boat was raised in 1887, and is now in the Museum of Oceanography in Berlin.

The Phillips boat, built on Lake Erie in 1851, was the forerunner of Simon Lake's boat. His boat was designed with wheels to run on the bottom, and it was intended for salvage purposes. The boat was propelled by steam and was fairly successful, but it had a shifting center of gravity, and on that account it was finally abandoned.

The Confederate submarine boat, the *Huxley*, was the first submarine up to the time of the Russo-Japanese war that ever destroyed a battleship. It was constructed of boiler iron, and had a crew of eight men. Her ballasting system was the same as used by Bushnell. A crank-shaft ran the length of the boat, and the crew turned it and thus propelled the boat. There was an officer stationed in each conning tower—fore and aft. Upon the first attempt to dive this boat was lost with her whole crew. The boat was in an awash position and had not left the dock when an excursion boat passed setting up a wash which passed through an open hatch and flooded the boat. The boat was later raised and equipped with a torpedo—a boom carried forward, on the end of which was a small contact mine. The mine was to be exploded against the side of the ship attacked. Unfortunately on one of their trials they became entangled with some growth on the bottom, and the man-power of the propeller was not sufficient to back the boat out. When they found the boat at the end of a week, of course, the men were all dead. Later another test was made, but the lieutenant in the forward conning tower stumbled and knocked the diving rudder control, causing the boat to suddenly dive while the conning tower was open, and all hands again perished. Finally, on February 17th, 1863, Lieutenant Dickson, of the Confederate Navy, decided to attack with this boat the *Housatonic*, Union sloop-of-war, then blockading Charleston harbor. He started out with eight men, and they got within a couple of hundred feet before they were discovered, when the sentry gave the alarm and fired at the boat. It was too close then to sheer her off, and a couple of seconds later the torpedo exploded against the *Housatonic* and she went down. The *Huxley* went down also, because her hatchway was open.

My father became interested in the subject of submarining when teaching school in Ireland in 1863. He read of the battle between the *Monitor* and the *Merrimac* at Hampton Roads, and he saw, of course, that the ironclad had come to stay. He set about to devise some weapon—something new—that could successfully combat an ironclad, and he finally decided that the submarine was the thing. He drew a plan for a boat at the time, but, of course, as is often the case with inventions, everybody thought it was nonsensical, and he had to abandon it. It was not until he came to Paterson, N. J., in 1871, that he succeeded in obtaining capital to build a small boat which he tried out on the Passaic River. It was designed to carry only one man. This boat was not armed nor fitted with a tube, but she had one great defect, as he found later. He had placed his diving rudders amidship instead of aft, as they should have been. His motor, which was supposed to be a petroleum engine, would "freeze" as soon as it got hot. He finally decided that it would be cheaper to build a new boat than to rectify the mistakes in the old one, so they took out the engine and fittings and sank the boat in the Passaic River.

This boat, the *Fenian Ram*, was built at the foot of West Thirteenth Street, New York, in 1881. The money was appropriated by the Fenian Brotherhood. The idea was not, as some people supposed, to build a boat to destroy the English navy. At that time certain claims between the United States and England had not been definitely settled, and there was some talk of war. This boat was to cruise in the Atlantic lane between Canada and England to destroy what traffic she could. The boat was 33 feet long and carried a crew of three men—the pilot, engineer, and the gunner. She was propelled by a Brayden petroleum engine of 30 horse-power—the first successful one ever built. Beneath the floor were the ballast tanks and valves. There were no air flasks on this boat; instead, two large compartments, one forward, the other aft, were used. From both the compartments air lines ran to the torpedo tube forward and supplied air to the interior of the boat and to the engine, and to expel water from the ballast tanks. The diving rudders were placed aft.

This boat was first tried in Morris & Cummings's breakwater in Jersey City. The first diving tests were very successful, and proved that the boat could do everything claimed for it. A short time after her first dive a reporter from the *New York Sun* visited my father and requested that he be allowed to inspect the interior of the boat, but permission was not granted; so the reporter returned to New York and wrote a very elaborate article on the "Fenian Ram," and that boat was ever after known by that name. The boat had no periscope, so it was necessary to come to the surface and then dive and run on dead reckoning. This was only the work of a moment, however, as the boat was very easily handled.

There is a rather funny incident I should like to tell in connection with this bow tube, the first time it was tried out. The test was made in Morris & Cummings's docks in Jersey City. Captain Ericsson, who designed the *Monitor*, was at that time building his destroyer, and he offered my father the use of two or three projectiles to make the test. My father accepted his kind offer, and they submerged the boat three feet to fire the first shot. The projectile traveled 30 feet from the nose of the boat; rose up in the air about 40 feet; came down, buried itself in the mud, and was never found again. The second projectile traveled about the same distance; cleared the water; went over the breakwater bounding the basin, and struck some piling on the end of a pier, behind which a man was sitting, fishing. Fortunately, the man was on the right side of the pile. Later, when they fired a different type of projectile, it proved very successful.

In 1895 the United States Navy Department advertised for plans for submarines. Three hundred thousand dollars had been appropriated to construct a submarine boat built in accordance with the best designs submitted in competition. My father submitted plans which were finally accepted. This boat was built from the money appropriated by the Government at the time, and was known as the first *Plunger*. She was built by the Columbian Iron Works, Baltimore, Md., in 1894, and was propelled by steam engines.

After the *Plunger* was abandoned, the Holland Company went to Mr. Lewis Nixon, who was at that time the proprietor of the Crescent Shipyard, in Elisabethport, N. J. He said that he would build a hull under certain conditions. My father said: "All right, go ahead," and construction was started in the fall of 1897. Before the work progressed very far a wealthy woman of New York gave us $25,000 to defray expenses.

This boat was launched shortly after the destruction of the battleship *Maine*, and it was pretty closely watched during that period. Her first trial was made on St. Patrick's Day, 1898. When they first tried to submerge it was found that the boat had too much reserve buoyancy, so they took pig iron on board for ballast. From that time on the eyes of the world were concentrated on her as really the first successful submarine boat.

The boat was propelled by a 50-horsepower gasoline Otto engine. When the boat was nearly completed they despaired of getting a suitable engine. My father happened to go to an electrical show in Madison Square Garden, where he saw an exhibit of an electric lighting plant for a country home. The generator was driven by a 50-horsepower Otto gasoline engine. He said, as soon as he saw the engine: "That is what I want for my boat," and he purchased the engine, which was placed in the *Holland*. In the other seven boats constructed for the United States Government Otto engines were installed.

During one of his dives in the lower New York Bay my father came near colliding with a lumber schooner. He came up just in time to see that he was pretty close, so he immediately dived fifteen or twenty feet to clear her. The following day a man came to see my father in his office in New York, and said, "You are Mr. Holland, are you not?" My father said: "Yes." "Well," said the visitor, "I am the captain of the lumber schooner"—naming her and stating where she was from, and so forth—and he said: "Yesterday you dived under my schooner as I was coming up the Narrows and your boat struck the bottom of my boat and seriously damaged her copper bottom, and I want to collect damages." My father replied: "Well, if such a thing had been the case your copper bottom would have ripped off the top of my conning tower, and I would not have been here to talk to you now."

After a dive the men lose no time in getting out of the boat for some fresh air, as there are all sorts of odors to be encountered in the interior of the boat; and, even though they have a plentiful supply of air on hand, it is not as pure as the air outside.

After the *Holland* had been accepted by the United States Government, a contract was made with the Holland Company for seven more submarines to be built on the same lines as the *Holland*.

At the time my father was building his first boat in the Columbian Iron Works, Baltimore, Md., Mr. Simon Lake was constructing his first boat, about 25 feet to one side of my father's boat. Mr. Lake's boat was designed for salvaging purposes and dived on what we call the

*Presented at a meeting of Engineers' Club, of Philadelphia, April 17, 1917, and republished from the "Proceedings."

"even keel" principle. Instead of having one set of diving rudders, she had two. In order to dive it was necessary to be moving with considerable speed and gradually tip the diving rudders, or planes, as he called them.

This was the first boat Mr. Lake built, and he did considerable salvage work with it around Baltimore. The boat was not designed as a war submarine, and he did not take up that subject until later on.

Should the Government Build Wooden Ships?

CONSIDERABLE attention has been given by publications of all classes to the question of our Government building wooden ships, some of the articles being signed by civil engineers, others by people who are in the habit of expressing their views on every topic of public interest. Of the former class, practically none have ever built a vessel of any kind, and although they may be experts in digging tunnels in the earth, in these days of specialization the mere right to sign "civil engineer" to their names gives no authority to their opinions on matters pertaining to ships. Of the second class, the quality of their utterances is exemplified by the argument of one such writer, who answered the statement that the timber for these ships is still growing in the forests by saying that "the ore for the steel ships is still lying in the mines." Unfortunately for his argument the passage of a tree through a saw-mill does not work the same change in quality that passing through the furnace does for the ore.

As illustrating the splendid supplies of material at our command for the project, the statement is made that an abundance of trees are to be had in the Douglas fir forests of the Pacific coast that are from 175 to 250 feet tall. This may be very true, but ships require special material that cannot be found in every tree, as is demonstrated by the fact that a representative of the coast lumber interests recently visited Washington and prevailed on the authorities to change the specifications for the proposed ships to permit of the 20-inch square keel 280 feet long, being constructed of four lengths of timber. Moreover he had the specifications for the planking changed to admit lengths of 40 instead of 45 feet. There is a considerable difference between these actual working sizes and a tree of 250 feet, as it grows in the woods.

To give an idea of the difficulty in securing timber for ships, one of the leading publications of the lumber trade of the Pacific coast recently quoted a representative of a number of big mills as saying that only 20 to 25 per cent of the lumber turned out in that region was suitable for the purpose.

If such difficulties in procuring suitable timber exist on the Pacific coast, where the forests are unsurpassed in the world, they certainly prevail to a greater degree in the South Atlantic States, to which is added an entire lack of timber with curved grain, so necessary for the frames and knees.

The matter of rapid decay resulting from the use of green wood is passed over lightly as of little consequence, but anyone who read the article in the issue of the SCIENTIFIC AMERICAN SUPPLEMENT of May 19th will realize that it has a considerable bearing on the subject. The cases cited in that article are defects arising in buildings on land, and incidentally call attention to the fact that even experienced engineers often make mistakes; but the case of ships is much more serious than land structures, for they are constantly moist, and the interior is always in a condition to favor rapid destruction. The ships 60 years old that are often cited, moreover, have all been rebuilt to a greater or less extent, and in many little of the original timber remains.

The general impression prevails that a wooden ship can be turned out in a few weeks, but in the contracts so far given out the time specified for delivery of the first ship is from eight to ten months. Compare this with the fact that steel vessels, of about the same size, have been launched from American yards this year six weeks after the keel was laid down; and an equal length of time would see them finished, equipped with machinery and ready to sail. This is three months as against eight for the wooden ship.

The reports from Washington make it evident that some of the parties most insistent on the Government building an immense fleet of wooden ships are promoters who are anxious to serve their country at a good round profit. Most of these parties do not at present own an inch of land, nor a splinter of timber, and of course they are entirely lacking in experience. All they have is a company on paper; and moreover, some of these companies have no intention of building the ships themselves, for a fat contract for several millions of dollars is an asset that can be readily disposed of to other speculators for cash.

The most insistent of these patriots also object to the idea of taking a contract for a fixed sum for each ship, but demand the condition of "cost plus 10 per cent." At first sight this appears quite fair, but there is nothing to prevent them from buying material from themselves—under another name—and in various similar ways multiplying the profits, while the country, as usual, pays the bill.

Another risk in entrusting the building of these ships to inexperienced contractors is, that the work is apt to be very poorly done—if for no other reason than the impossiblity of securing competent workmen. For the same reason inspection would be valueless because experienced inspectors are not available. If for any reason the ship is poorly constructed there is little danger of the fact ever being found out, for the craft will shake to pieces in the first heavy sea, and there is no evidence left to prove the case. More than one dishonest builder, in the "good old days," took chances of this kind; and inexperienced builders, who did not appreciate the overwhelming power of the sea, slighted their work through ignorance, with most disastrous results to all parties concerned.

The "splendid service" rendered by the sailing fleets of by-gone years is frequently cited; but they were all we had then, and as soon as steel building and steam-power were developed the wooden ship began rapidly to disappear, not because the steel ship was cheaper, but because it was better in every way. Compared with modern steel-built vessels the service of the wooden ship was decidedly undesirable.

From every point of view the proposal to build a vast fleet of wooden cargo carriers would appear to be a doubtful way to sink the public millions, for sunk it surely would be by the elements, even if it escaped the submarines.

Making Lantern Slides from Line Diagrams

LEST it should be thought that the task of making a lantern slide from some line diagram in a book is too easy an operation to justify anything being written about it at all, the writer may be forgiven for pointing out that two of the worst series of slides that he has ever seen were of diagrams made and shown to illustrate deep papers upon photographic theories by men whose names were household words in photography. On the underlying principles of speed measurement and the latent image they were authorities, but when they had to make a line negative and a lantern slide from it the audience found that they were just human beings like the rest of us, and had struck the same sort of snags.

One of the troubles likely to be experienced in making such slides lies in the condition of the original. If drawings are to be made for the express purpose of making lantern slides from them, the draughtsman must be cautioned against the use of ordinary writing ink and drawing paper. The whitest "Bristol board" should be employed, and the work done on this in "artists' black"—liquid ink which can be obtained from any colorman. With such originals the work is plain sailing.

But if, as may easily happen, the diagram is in a book or newspaper, possibly dirty and creased, it may be a difficult matter to get a good result from it. Creases may be got out by damping, placing between clean paper, and ironing. Dirt will yield to gentle rubbing with bread crumbs, and grease may be removed by means of petrol. If the paper used to protect the print during the ironing is blotting paper, and it is changed once or twice during the operation, much of the grease can be absorbed in that way, but dabbing with cotton wool wetted with petrol is the quickest way to remove it. This should be done in the open air, or at least not anywhere near a naked light, as the vapour of petrol mixed with air forms an explosive combination.

If it is permissible, a crumpled original can be made perfectly flat by treatment in the following manner: A sheet of paper decidedly larger than it is made thoroughly damp, and the original, being well pasted on the back, when quite limp, is laid down upon it and rubbed well into contact. The edges of the sheet of paper are themselves pasted at the back,and it is stuck down by means of them to a stout card or board. As it dries it will stretch itself perfectly flat and as tight as a drum, and the original will be flattened at the same time. This is then a very convenient time to undertake any cleaning that may be necessary, as the original is held firmly, and, being backed up, will stand more rubbing.

If slides are to be made from illustrations in books, the proceedings must be a little different. To flatten the page it may be placed between a couple of pieces of glass, which are held together by clips or by elastic bands. The page is then propped up vertically to be photographed. In doing so, it is well to throw a black cloth over the page which lies in front of the glass, and indeed over the abtle generally in that neighborhood, to avoid awkward reflections from the surface of the glass itself. If for any reason it is undesirable to have glass over the diagram while copying it, a board or even a stout piece of card may be provided, with an opening in it, a little larger than the diagram, but not so large as the entire page, and the page brought up behind this, with glass or another board behind it, and the two clipped together as just described.

In some circumstances any printing on the back of the original may show through and give trouble by appearing in the copy. This can be prevented by backing up the page with a sheet of black paper, made by giving a coat of matt black varnish to ordinary paper. It has been said that a copy is sometimes improved, when the ground is not very even and white, by putting in front of it a piece of finely ground glass, but the writer has had no experience of this.

Much of the success in negative making will depend upon the illumination. On the whole, artificial light is to be perferred, using two lamps, one on each side of the camera, and as near together and to the original as can be managed. The lens must be carefully screened from any ray of direct light from the lamps. Incandescent gas, if it is available, is an excellent illuminant, constant and powerful, but even oil lamps will be found to answer very well. A full exposure must be given, followed by very full development. As the negative is to have only two tones, representing black and white, it will be found helpful to use a developer of full strength and to add to it, say, a grain of potassium bromide to each ounce. This will help to keep the lines clear. Slow plates, those known as "ordinaries," are best for such work, but at a pinch very good results can be got on extra rapid ones, or on roll film. Plates for line subjects should never be used unbacked.

When the negative has been fixed and well washed, but not before, it should be given a brief immersion in a ferricyanide and hypo reducer, and immediately well rinsed. The effect of this will be to clear the lines without appreciably weakening the density of the groundwork. If the one can not be done without the other, then the lines may be made quite clear, regardless of the density of the rest, and the negative intensified with Wellington's silver intensifier until the ground work is once more opaque.

For many purposes the negatives made in this way can be used as slides in the lantern in place of positives. They give the diagram in light lines on a black ground, which will be found much less fatiguing to the eyes than black lines on a clear ground, but if a pointer is to be used, it is not so visible. If the negative forms the slide, care must be used to see that it is spotted in such a way as to bring the reading matter the right way round and not reversed. If slides are to be made from the negatives, backed lantern plates may be used. The opacity of the negatives and clearness of the lines should make the work very simple and straightforward.—*H. M. R. Scott in Photography and Focus.*

Peat as Locomotive Fuel

REPORTS from Sweden appear to be very favorable as regards the use of peat as a fuel for steam locomotives, the peat being dried and powdered, and employed in the latter state, which is affirmed to be the best for firing locomotive furnaces. The government railroad administration appointed a commission within a recent date in order to make a thorough examination of powdered peat as a fuel, and after a series of tests upon the preparation and combustion of this substance, the commission reported in its favor, stating that the results were conclusive. The report established the fact that powdered peat can be utilized as a good source of fuel and concluded that it should be employed on a large scale.

Accordingly the Swedish government is taking measures to erect a plant of considerable size near the peat fields of Lake Vetter for the purpose of making peat dust. As to the use of this fuel on locomotives, it is stated that all the locomotives on the 60-mile Falköping-Nassjo railroad line are now running on peat fuel with great success. The official tests showed that peat fuel as regards calorific powder (by weight), is about two-thirds the value for coal. The results which are already attained are claimed to justify the erection of large plants to utilize this national resource. In order to keep this production clear of complicated questions regarding competition from coal, the government decided to have the new plants operated by the state, at least for the present, and is now engaged upon the plans for utilizing several large peat beds. For instance the Hästhagen fields would afford some twenty thousand tons of powdered peat per annum, this to be used for the railroads, and on this basis the beds would not be exhausted before 20 years. After the peat is removed, the ground can be used to advantage as farm land.

Photo by Dr. Alfredo Jahn, Caracas

Highland type of Indian woman from region of Mérida

Photo by Herbert J. Spinden

Lowland type from Goajira peninsula. The women have adopted voluminous, balloonlike gowns, but still tattoo the arms, and wear their long strings of beads under the gown

The Parajuano woman seated in the hammock, has her face painted in a fashion peculiar to this tribe

Travel Notes in Western Venezuela*

A Picturesque and Primitive Region

By Herbert J. Spinden

It is a time-honored story that relates the origin of the name Venezuela and one more to be credited than the majority of place-name anecdotes. When Alonzo de Ojeda, accompanied by the illustrious geographer, Amerigo Vespucci, entered the Gulf of Maracaibo in 1499, he found villages raised on piles above the water. From this circumstance he gave the name "Gulf of Venice" to the shallow sea enclosed between the arid peninsulas of Goajira and Parajuano. Those were days of flowery speech and fervid imagination: the comparison between Venice the Magnificent, and the homes of humble fishing Indians in the New World, struck the popular fancy and soon the whole land was known as Venezuela—Little Venice. The surviving pile-built villages north of the city of Maracaibo are still of great interest to the ethnologist and to the traveler with an eye for the picturesque.

Slipping out of Maracaibo at nightfall in a *bongo*, manned by Parajuano Indians, we arrived at the lagoon of Sinimaica at ten o'clock next morning. The *bongo* is a flat-bottomed boat propelled by sails or poles and fit for navigating shallow waters. The lagoon of Sinimaica is the largest of a series of small brackish lakes connected by natural canals called *caños*. There are three villages of Parajuano Indians in this lagoon bearing the Spanish names La Boquita, La Boca del Caño, and El Barro. All are of the same character, being composed of detached clusters of houses well out from the low shore. Mangrove thickets fringe the open water where they have not been cleared away for coconut walks. There is a tide of perhaps two feet in the lagoon and the houses rise two or three feet above the high-water mark. All traffic is by canoe and you step from the wobbly dugout upon ladders rising from the water and find yourself on a shaky platform of small poles. You are courteously invited to enter. The rectangular houses have light frames and roofs of heavy thatch. Mats enclose the sides and cover portions of the floor. The fireplace is a box filled with earth. While you sit on your heels and eat toasted plantains and boiled manioc, you see through the latticed floor the upturned faces of little scavenger fishes eager to catch the crumbs that fall.

*From the *American Museum Journal*, January 1907 (published by the American Museum of National History, New York). Illustrations by the Author.

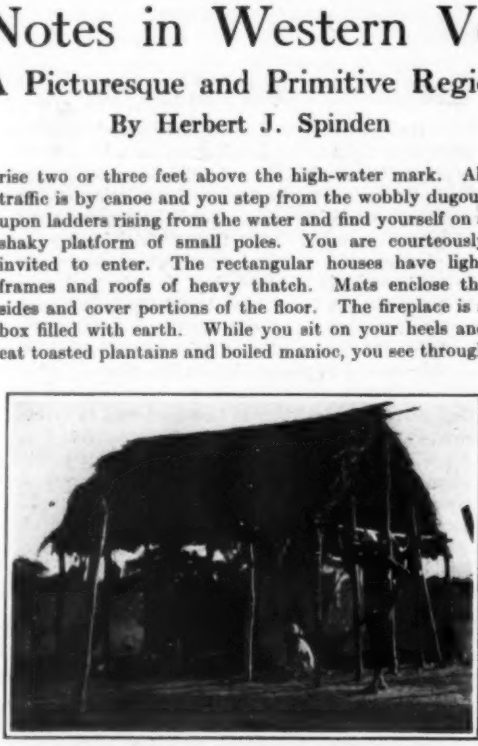


Photo by Herbert J. Spinden

Open house is kept in the desert by the Parajuano Indians. The old man wears the apron of olden times

Freedom from the insect life that makes the shores unbearable may account for the custom of building houses out over the water. But it is an interesting fact that these lake dwellers also have houses upon the arid plain well back from the thicket-covered shores. The plain is a dreary stretch sparsely covered with acacias and other desert shrubs. The most conspicuous of these is the divi-divi from the seeds of which a valuable dye and tanning substance is extracted. The houses in the desert are mere roofs upon poles and as often as not the hammocks are swung under an unusually large tree. The natives take evident delight in keeping open house and in living *al fresco*.

The dress affected by the women at the present time is a voluminous gown resembling a partially deflated balloon. In ancient times the dress was doubtless much simpler and it is interesting to note that bandoleers and belts made of long strings of beads are now worn under these generous garments. Tattooing is seen upon the arms while the face is ordinarily painted with a large circle upon each cheek connected by a line across the nose. The men are sometimes seen in their ancient attire, which consists of a belt and breechcloth and poncho. Artificial wigs are worn by the leading men, and sandals with tasseled latches.

The Goajiro Indians live mostly in the interior of the desert peninsula that bears their name and their warlike habits have kept the white man from their territory. In two places they have come in contact with civilization, one at the Catholic mission of Rio Hacha in Colombia and the other at the lagoon of Parawaipowa where the Venezuelan government maintains a border garrison. The mountain range west of Lake Maracaibo (the Sierra de Perihá) is controlled by the wild and little known Motilones.

In Venezuelan histories one reads dark tales of Sir Walter Raleigh, L'Olonais, Sir Henry Morgan, and the lesser buccaneers who ravaged the Spanish Main for the glory of England and their own immediate fortune. Maracaibo was sacked again and again. Even the strongly defended Gibraltar at the southern end of Lake Maracaibo was destroyed. Today one sees at the latter site a few modern huts built around the old plaza. Paved streets can be traced out into the bush and ruined walls enter the waters of the encroaching lake. Only a graceful bell tower remains intact from former times.

Lake Maracaibo is surrounded by a coastal plain extremely dry in the north and given over to cactus and thorny shrubs, but humid in the south and clothed in heavy forest. The sierras rise abruptly from the edge of this plain and to great heights. From the southern end of the lake the Andes seem an impassable wall with their forested slopes and fogbound crests. And indeed the trails that sidewind deep gorges and climb lofty ridges, only to drop again to the roaring stream, find passes in the barren paramo some fourteen thousand feet above the sea. The transitions from one type of environment to another are sudden and startling.

Photo by Herbert J. Spinden

The principal plant of the paramo of the Andes—the land above timber line—is the Espeletia, with leaves heavily belted against cold with down that resembles cotton

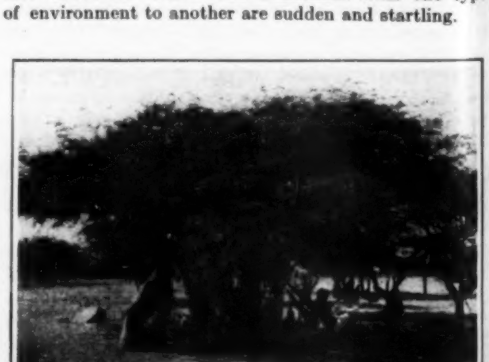


Photo by Herbert J. Spinden

On the arid plains back from the lakes a large divi-divi tree often serves as the sole shelter of a family


Photos by Herbert J. Spinden

Among the lake dwellers in Venezuela there are considerable differences in wealth. The poorer people live in picturesque poverty in an outer line of houses

The pile-built villages in the lagoon of Sinimaica are survivals of the ancient villages of the Indians from which the name Venezuela —Little Venice—was derived

A direct but little used trail for Mérida leaves the unhealthful lake port of Bobures, passes through small savannahs and stretches of dripping forest to Torondoy, a coffee center, and then ascends the Torondoy River to its very source in the paramo of Mucumpate. In the coffee region the mountain-sides have been cleared of forest and only the widespreading bucari trees retained as shade for the tender shrubs. In February these bucari trees are masses of vermilion blossoms.

Leaving Torondoy we soon find that the valley has become too dry for coffee, perhaps because the high ridges to seaward rob the winds of their rain. Small huts of mud and thatch cling to the steep slopes, and irregular fields of maize differing in age and condition make an odd patchwork of dull colors on the mountain-sides. The brighter green of plantains shows itself in moist ravines. At Mucumpis Below we are not able to wheedle a noonday meal from Mrs. Sanchez who keeps the posada. After a brief rest we climb in a dizzy zigzag to Mucumpis Above where half a dozen Indian huts are clustered in a hanging valley. We now enter a second zone of humidity and soon the forest closes in about us. While the trees are not of great size the growth is very dense, with vines, canes, and ferns competing for the scanty spaces between trunks and branches. One sees the familiar plants of New England gardens in begonias that bank the trail, in fuchsias, geraniums, and the purplish Wandering Jew. Out of the blue the afternoon fog creates itself in wisps and shreds and soon the world is lost to sight. The forest opens, for we are nearing its upper limit and the trail feels its way in the white dusk along the edge of things. Mossy trees are like vanishing ghosts but reality comes in the roar of the stream a thousand feet below.

At Samuro, a rambling mud-walled structure with smoke-blackened thatch, we found a number of shivering Indians in striped ponchos. The earthen floor of the common sleeping-room was wet and the chill fog penetrated everything. Kindliness and good humor flourish, but cleanliness is an unknown virtue in the mountain inns of Venezuela. One may easily picture some starving disciple of Hygeia shutting his eyes and praying while he eats, but to a peripatetic anthropologist after a hard day on the trail the earthy smell of the small greenish potatoes is grateful and comforting. Then there is a savory but uncertain stew and perhaps an egg sprinkled with the rusty salt that the government sells. On special occasions a medieval boar's head may be brought in on a charger.

The paramo is the barren land above the timber line. In the Venezuelan Andes it begins at about ten thousand feet elevation. The transition from the upper forest zone is fairly abrupt and is marked by the dwindling of the trees and by the increase in size of the plant most characteristic of the paramo—namely, the *Espeletia*. This curious perennial, with its leafy crown felted with cotton, against the cold, its awkward flower stocks set with yellow blossoms, and its thick trunk made of the dead and blackened leaves of former years, is called the *Frailijon del Paramo*—the Great Friar of the Paramo. It grows to the height of six or seven feet and in the distance is not unlike a human figure in white cowl and black cloak. Another plant of the paramo has small leaves braided along the close-growing stems. It might be mistaken for juniper were it not for the pink and yellow blossoms. A common bush resembles the huckleberry and is often heavily fruited.

From the divide one gains a splendid view of the Sierra Nevada de Mérida across the deep valley of the Chama River. This range boasts five peaks rising into the zone of perpetual snow. The highest one, La Corona, is given by latest measurements an altitude of 5,002 meters (16,411 feet). The trail drops down through a valley

Photo by Dr. Alfred Jahn, Caracas

A street scene in Mérichia. Five snowy peaks rise directly opposite this city and afford a brilliant contrast of tropical and arctic life

showing signs of ancient glaciation and after two hours' travel we come to a region where wheat is raised. The little irregular fields are as stony as those of the New Hampshire hills. At Mucuchies, a town of some size

Photo by Herbert J. Spinden

Weaving cotton is still practiced by the Goajira Indians, who make sashes, ponchos and hammocks

with a population largely Indian, our trail joins with the better traveled one running from Valéra to Mérida.

The valley of the Chama below Mucuchies grows drier and hotter as we go down it but before Mérida is reached conditions change for the better. Mérida is an old-fashioned city and a natural fortress, placed on a sloping bench between two streams. The University of the Andes is situated here. Coffee of superior quality is grown as well as a variety of other tropical products.

From Mérida one may travel by mule to Valéra, Trujillo, Carache, and Tucuyo in a general northeasterly direction. The Andes decrease in elevation but some of the passes are ten thousand feet or more above the sea. There are narrow forest belts as well as short stretches of true paramo. The lower country is hot and dry and when we come to the broad plain in which lie Tucuyo and Barquisimeto we find forests of cactus trees made almost impenetrable by underbrush of smaller cactus. Only where water can be put on the dry soil is agriculture possible.

The archeological explorations were extended into northern Venezuela. Next a journey was made southward across the llanos or grassy plains to Cabalozo and San Fernando de Apure.

As a purely scientific result of the expedition, it now seems clear that the earliest pottery art of Mexico and Central America, belonging on what is known as the "archaic horizon," was extended in ancient times across northern South America possibly to the mouth of the Amazon. The trail, marked by clay figurines of a peculiar style, is of greatest interest because the spread of this pottery art was associated with the first spread of agriculture, and with the historic development of plants, such as maize and beans, that are of the utmost importance in our own day and civilization.

The Effect of Small Amounts of Impurities, in Particular Chlorides and Sulphates, in Producing Turbidity or Opacity in Glass

At a meeting of the Society of Glass Technology (England) on March 25th, a paper on the above subject was presented by W. E. S. Turner and J. D. Canwood, of which the following is an abstract, reported by the *Chemical News:*

The work originated in troubles that had been experienced by manufacturers using Russian potash as one of the components of a lead batch. The glass made with Russian potash though found at first to be quite clear gradually became milky as it stood at a lower temperature in the pot, or if after the glass had been worked the articles in course of formation were reheated. Analysis indicated that the three impurities in Russian potash to any extent were phosphate, chloride and sulphate. Of these three, phosphate was not active in producing milkiness, at least up to 1 per cent of the batch mixture, but chloride and sulphate did cause opacity. Of the two, sulphate was the more powerful, and even potassium sulphate to the extent of ¼ per cent of the batch produced opacity, when melting at 1,300° was carried out until the glass was clear, followed by several hours' heating at 950—1,000°. Under the same conditions to give opacity, chloride must be present to an extent exceeding ½ per cent of the batch.

It was possible to prevent or retard the appearance of milkiness by the addition of borax. The most effective agent, however, is high temperature, by which chloride is volitilized or sulphate decomposed. The results emphasized the need of the introduction of more efficient furnaces, capable of reaching a high temperature. Especially was this so when dealing with the new types of glassware that now had to be produced in this country.

In the discussion that followed, it was pointed out that while borax removed the milkiness, on the glass being reheated it again appeared. Thus, the best means of removing this defect was to employ a high temperature.

Pressure Phenomena Accompanying the Growth of Crystals*

By Stephen Taber

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UNDER suitable conditions crystals grow in directions in which growth is opposed by external force. This fact appears to have been first observed by Lavalle in 1853.[1] It was denied, however by Kopp, who, after making certain experiments, stated that he was never able to observe anything tending to confirm the view that a crystal can raise itself in order to grow also on the side on which it rests.[2] Subsequently the observations of Lavalle were confirmed by Lehmann[3] and others.

Becker and Day seem to have made the first attempt at determining the magnitude of the force developed during crystal growth. In their experiment, a crystal of alum supporting a weight was covered with a saturated solution of alum, and supersaturation was induced by evaporation. The crystal increased in size through growth on the lateral exposed faces which were also extended downward, thus lifting the crystal together with its load. The deposition of new material on the lower surface was restricted to the periphery, so that a hollow face was gradually formed by the downward extension of the new growth, and the crystal rested on a very narrow outer rim. The area of this rim was determined with difficulty, but repeated measurements led to the conclusion that "the force per unit area which the crystals exert . . . is of the same order of magnitude as the ascertained resistance which the crystals offered to crushing stresses."[4]

In 1913 Bruhns and Mecklenburg published a paper[5] in which they claim to have repeated the experiment of Becker and Day with negative results. In a reply to this paper Becker and Day explain the different results obtained by Bruhns and Mecklenburg as due to the fact that the original experiment was not duplicated.[6] This conclusion was reached independently by the present writer.[7] By placing an unloaded crystal in the same solution with the loaded one supersaturation with respect to the supporting surface of the loaded crystal was prevented.

Bruhns and Mecklenburg[5] (pp. 106-108) describe another experiment in which the crystallization of chrome alum resulted in raising porcelain fragments, loaded with weighted beaker-glasses, but they thought it essential that evaporation be carried to completion. The elevation of these beakers and similar evidences of pressure phenomena accompanying crystal growth they attribute to the "forces of adsorption and capillarity" and not to a "force of crystallization." Becker and Day show that in this experiment the lifting action occurs in spite of capillary attraction rather than because of it, and that adsorption merely diminishes the rate of growth by delaying diffusion.[8]

As a result of their investigations Becker and Day conclude: (1) that there is "a linear force, apart from the volume expansion, exerted by growing crystals"; (2) that this force enables them to grow in directions in which growth is opposed by external force "notwithstanding unrestricted opportunity for growth in other directions; and (3) that the linear force thus exerted is of the order of magnitude of the breaking strength of the crystal."[9] "The crucial experiment (briefly described above) offered in support of this conclusion" is not, in the opinion of the present writer, decisive. It is significant that no growth occurs on the upper face of the crystal although it is subjected to less pressure than the lower face, and crystallographically both are the same. The elevation of the crystal is due to the fact that it rests directly on a thin layer of solution which is supersaturated by diffusion from without; and in this experiment the solution at the bottom of the crystallizing dish is of higher concentration than elsewhere. Furthermore, the effect of expansion in volume is not eliminated in this experiment, for alum separates from solution with increase in volume.

A crystal is enlarged through the addition of layers of material to its outer surfaces, and this takes place when these surfaces are in contact with a supersaturated solution. A very thin coating of impervious material is sufficient to prevent crystal growth. In the Becker and Day experiment the supporting edges of the crystal rest directly on a thin layer of solution which is therefore under pressure. Now pressure tends to reduce the thickness of the supporting film to a minimum. With perfectly smooth parallel surfaces the minimum thickness of the separating film is perhaps equal to the diameter of the space occupied by a molecule of the liquid, and this is probably approximated in the present case, for deposition would be most rapid where the thickness of the supporting film is greatest. It would require great pressure to completely expel the solution from such a narrow space, and, if the solution is thus excluded, growth in a vertical direction would cease. The fact that this growth continues is proof that the solution is not expelled from under the crystal. Therefore, when alum separates from solution and is added to the base of the crystal, the accompanying increase in volume must result in some elevation, irrespective of other causes.

The cavity or hollow formed on the under side of crystals in the experiment described above results from malnutrition, due to the slow rate of diffusion under the crystal as compared with the relatively rapid growth in other directions. The writer succeeded in eliminating this cavity by supplying the supersaturated solution through capillary openings under the base of small crystals. Since new material can be added only at the base, its area remains small and tall slender columns are formed.

The pressure phenomena observed during the growth of crystals have been attributed by the present writer "to the molecular forces associated with the separation of solids from solution and to the attraction and orientation of the physical molecules as they are brought into position on the surface of a growing crystal"[7] (pp. 553-554). He suggested "that the force is due chiefly to the expansion in volume which accompanies the separation of most solids from solution, for, as yet, he has obtained no pressure effects during the crystallization of substances that separate from solution with decrease in volume."[10] Recently, however, he has obtained definite evidence of pressure accompanying the crystallization of ammonium nitrate.[11] The experiment is described below.

A cup of porous porcelain was placed bottom up in a small jar half full of a concentrated solution of ammonium nitrate, and a piece of parafine-coated cardboard, cut to fit snugly around the cell, was cemented with paraffine to the top of the jar and to the walls of the cup. The jar was then placed in a desiccator containing calcium chloride, and allowed to stand undisturbed for nine months. The solution was drawn by capillary attraction to the upper and exposed walls of the cup where a crust was gradually formed by evaporation, but this crust was enlarged chiefly or entirely through the addition of new material to the outer exposed surfaces, the solution reaching these surfaces through capillary pores in the crystalline mass. Under the card-board, however, a few long acicular crystals were formed on the surface of the cup, and these were gradually pushed outward by the addition of new material to their base. At one place the solution penetrated the cardboard which was gradually split apart by the slow growth of a lens-shaped veinlet of finely crystalline salt about 3 mm. in thickness. The cup was not broken as in similar experiments with certain other salts that separate from solution with increase in volume.

The pressure phenomena observed in this experiment are explained as follows: Crystallization is retarded or prevented in supersaturated solutions which occupy small capillary or subcapillary spaces; therefore crystals may be supplied with material for growth by diffusion through solutions occupying such spaces, and the increase in volume due to the entrance and deposition of new material must result either in the expulsion of part of the solution or in the enlargement of the space occupied by the growing crystals. If the spaces occupied by the solution are relatively large, the solution will be gradually expelled as crystalline matter is deposited in its place, but, on the other hand, if the spaces are sufficiently small, less force may be required to enlarge the space occupied by the growing crystals than is necessary to expel the solution. The diffusion of a solid through a solution and its separation therefrom are attributed to osmotic pressure and the relation between osmotic pressure and solution pressure. According to the writer's theory, the force observed in this experiment with ammonium nitrate is analogous to the pressure developed when an anhydrous salt, confined in a limited space, combines with water that has diffused as vapor through capillary openings.[12]

Molecular attraction between solid and liquid causes the thin layer of solution in contact with a crystal to adhere to it; and this contact film, on account of adsorption, is of different concentration from the bulk of the solution. Enlargement or solution of a crystal is brought about by the diffusion of dissolved substance across this layer. When the solution in contact with a crystal surface is supersaturated with respect to that surface, new material is deposited on the crystal, thus forcing the contact film to move outward from the growing crystal. If this film approaches a foreign body, growth in that direction is gradually retarded as the space through which diffusion must supply new material becomes more limited. Consequently growth will be more rapid in other directions, providing diffusion is not similarly restricted, and the crystal will tend to surround the foreign body. Deposition of material between the crystal and the foreign body will continue, however, as long as diffusion can maintain supersaturation in the solution occupying this space; and, with continued crystal growth, the contact film must continue to be displaced. When this film comes in contact with the foreign body, any further growth must result in either (1) the displacement of the foreign body or (2) the rupture and expulsion of the film; and the outcome will depend on the resistance offered by the foreign body, the dimensions of the space occupied by solution and the mutual attraction between the molecules of the liquid and solids.

If the crystal is of a substance that goes into solution with decrease in volume, increased pressure will make it more soluble, thus increasing the degree of concentration requisite for further growth; but for those salts that have been tested a large change in pressure is required to produce an appreciable change in solubility. If the solubility of the foreign body is increased by pressure, it may be gradually removed in solution as the growing crystal replaces it.

The tendency of a crystal to assume a regular polyhedral form is important as a factor in the development of pressure during crystal growth only in so far as it affects the relative solubility of the crystal in different directions. A crystal growing in a solution of uniform concentration tends to build that form which is the least soluble under existing conditions, or in other words, for which the total surface energy is a minimum. If the surface tension were the same in all directions this form would be a sphere, but in crystals the surface tension differs in different directions, or, on different faces, and is the same only on faces that are crystallographically the same. A solution that is in equilibrium with the flat face of a crystal will be supersaturated with respect to a concavity on the face and undersaturated with respect to a convexity. When a crystal having a concave face or an artificially truncated angle is placed in a solution of uniform concentration which is kept saturated with respect to the normal crystal faces, growth may be limited to a single direction until the imperfection is repaired, but growth can not continue indefinitely in a single direction as the superficial area and hence the surface energy would increase too rapidly in proportion to the volume. The variation in the solubility of a crystal in different directions is slight, and therefore it is probable that the difference is small in the pressure that may be developed in different directions by a growing crystal in contact on all its surfaces with a solution of uniform concentration. The surface tension on the different faces of a growing crystal probably depends on many factors such as the number of molecules per unit area, their structure and orientation, pressure, temperature, and the composition of the solution. Hence, different faces may develop under different conditions of growth.

Most of the phenomena hitherto cited in support of the hypothesis that there is a "linear force of crystallization" are to be explained by the fact that the growing crystals have been in contact with a supersaturated solution in only one direction or that the concentration of the solution has been greater in one direction than in others. It is probable that the pressure effects observed during crystallization are due chiefly to the separation of solid matter from solution rather than to the growth of crystals, and, under favorable conditions, the pressure developed in this way may greatly exceed the crushing strength of the substance.

Crystals grow in directions in which external forces oppose growth whenever the surfaces under pressure are in contact with a film of supersaturated solution, and it is possible to supply the material for growth by slow diffusion through subcapillary spaces, as great resistance

*A paper communicated to the National Academy of Sciences, and republished from the *Proceedings*.

[1] Lavalle, J., *Paris, C.-R. Acad. Sci.*, 36, 1853, (493).

[2] Kopp, H., *Ann. Chem. Pharm., Leipzig*, 94, 1855, (124).

[3] Lehmann, O., *Molekularphysik*, 1, (342), *Leipzig*, 1888.

[4] Becker, G. F., and Day, A. L., *Proc. Washington Acad. Sci.*, 7, 1905, (285-287).

[5] Bruhns, W., and Mecklenburg, W., *Jahresber. Niedersachs. geol. Ver. Hanover*, 6, 1913, (22-115).

[6] Becker, G. F., and Day, A. L., *Chicago, J. Geol., Univ. Chic.*, 24, 1916, (315-325).

[7] Taber, S., *New Haven, Amer. J. Sci.*, (Ser. 4), 41, 1916, (535).

[8] Becker, G. F. and Day, A. L., *Chicago, J. Geol., Univ. Chic.*, 24, 1916, (325-329).

[9] *Ibid.*, (313).

[10] Taber, Stephen, The origin of veins of the asbestiform minerals, these Proceedings, 2, 1916, (662).

[11] According to Traube ammonium nitrate goes into solution with expansion in volume; *Zs. anorg. Chem., Hamburg*, 3, 1893 (1). This reference cited in G. P. Baxter's Changes in volume upon solution in water of the salts of the alkalis, *J. Amer. Chem. Soc., Easton, Pa.*, 33, 1911, (923).

[12] For description of this experiment see The genesis of asbestos and asbestiform minerals by Stephen Taber, *Bull. Amer. Inst. Min. Eng.* No. 119, p. 1987, Nov. 1916

offered to the expulsion of solution from such openings. he conditions requisite for the growth of crystals under ressure commonly obtain in the rocks of the earth's rust, and many phenomena connected with the meta-orphism of rocks, the growth of concretions, and the rmation of mineral deposits are difficult of explanation nder any other hypothesis than that growing crystals ave made room for themselves by exerting pressure on e surrounding material.

The Principle of Relativity*

By Alph. Berget

It was some twelve years ago (1905), that an ab-olutely new doctrine in the physical sciences first saw e light of day: we refer to the *principle of relativity*, rmulated and developed for the first time by the erman, Einstein.

The concept of this doctrine, applied at first to space d to time, and afterward to all the other physical agnitudes, entirely changes the fundamental ideas pon which science has reposed until the present time, d the upheaval it thus produces has certainly no rallel in the history of human thought. It brings us ce to face with an aspect of the universe utterly dif-rent from that which has heretofore contented us: in is "new" aspect, in this unexpected image of the world hich we are thus led to form, the propositions which ave been regarded as *self-evident* verities, as axioms eding no demonstration, find themselves annihilated.

German science has literally "drawn the long bow" er this new principle which so profoundly disturbs e truths hitherto accepted in the domain of physics. erman science, in fact, has always been inordinately nd of developments starting from "hypotheses," en when these were absolutely gratuitous, and have but ight foundation in that "good sense," which funda-entally should be the unshakable basis of every body f scientific doctrine.

Let us begin by calling to mind certain definitions. et us designate by the term system A a certain number f material bodies and of geometric figures connected ith these bodies; let us suppose these bodies and these gures to be animated by *a common movement*.

Let us designate in the same way by system A' another oup of bodies and figures, likewise animated by a mmon movement. Let us imagine that, at a given oment, the *axes of the co-ordinates* to which, in the two stems, the positions of all their respective elements are lated, find themselves in coincidence.

In these conditions we give the term of the *relative ovement* of the two systems A and A' to the movement f one of the systems which is observed by a spectator re-ted to the other system in invariable manner. If, in is case, the speed of a rectilinear and uniform relative ovement v for a spectator placed in the system A, it ill be $-v$ for an observer placed in the system A'.

But this idea supposes that the two systems are cap-ble of being individually in movement. But the uestion may arise as to whether there exists an *absolute ovement*, that is, whether one of the systems under nsideration, which we will call A, could be in a state of *solute repose*. If such a system exists, the relative ovement of any system whatever with relation to A ould be the absolute movement of this system.

Does such a system, A, in absolute repose, exist? If , it is probably not found in the stellar universe, at st if we regard it as related to the earth, to the sun, or any celestial body whatever, since observation shows that all these bodies are in a state of perpetual dis-acement.

But we may regard from another point of view the istence of the system A in absolute repose.

Physicists have been led to believe, for nearly a ntury, in the existence of an elastic medium, the ether, rving as a vehicle for the vibrations of light and of ectricity, and expanded throughout all space and in all dies. If this ether really exists, if it fills the whole of finite space, and if we can regard it as *immobile*, at st outside of matter, we shall be able to consider that body at rest with regard to the ether will be in a state f *absolute repose* and that all movement related to the es of coördinates which are immobile in the ether will present an *absolute movement*.

But the *principle of relativity* involves the following w: *No experiment, either optical or electrical, made at e surface of the earth, can possibly prove the movement of anslation of the earth.*

We shall see that the principle of relativity brings to question the very existence of the ether.

If we admit the existence of the medium "ether": if, oreover, we consider it as absolutely immobile (the ypothesis of Lorentz). We deduce therefrom that *e absolute repose* on the one hand *and the absolute ctilinear movement must exist*. To renounce the idea of repose and of absolute movement would be equivalent therefore to renouncing the very existence of the ether.

On the other hand, in supposing the ether to be im-mobile and not carried along by bodies in motion, *i. e.*, in supposing that the ether contained within all bodies in nature in no wise takes part in the motion which may animate them, we should have to believe that the rectilinear and uniform absolute movement of a body, that of the earth, for example, manifests itself in the phenomena of propagation of electro-magnetic disturb-ances observed upon this body.

During a very brief interval of time we may consider the movement of translation of the earth as being rectilinear and uniform. We may take for the value of its speed v, the value 30 kilometers per second. The speed of propagation of electro-magnetic disturbances in the ether (speed of light), is equal to 300,000 kilometers per second. Let us designate this latter speed by c. We perceive that the ratio (v: c), is equal to 10^{-4} and the ratio $(v: c)^2$ is equal to 10^{-8}.

The influence of the movement of the earth upon electrical and optical phenomena should theoretically manifest itself by a variation of the numerical values of certain magnitudes, and it is easy to see that this vari-ation must be a function of the ratio (v: c) [actions of the *first order*], or of $(v: c)^2$ [actions of the second order].

Naturally, experimental verifications have been sought for, and in the first place there has been an attempt to prove actions of the first order.

For this purpose we observe an optical or electrical phenomenon which is propagated in a determined direction, at first when this direction is parallel to the movement of translation of the earth, afterwards when it is perpendicular to it or directly opposite.

Although numerous experiments have been made, no one has ever been able to observe any difference between the values of magnitude measured in the two cases.

As for actions of the second order, they have been studied experimentally by Michelson, Morley, Nieller in America, by Lord Rayleigh, Bruce, Frouton, and Noble in England. These researches are all recent; some (Michelson), are optical, the others are electrical. *All have given a negative result.*

It is in the presence of the negative results of these tests that the advocates of the principle of relativity have constructed their theories. Instead of confining themselves to the modest statement that *no one has as yet* been able to prove directly the influence of the move-ment of the earth, they assume in principle, that *no one can prove* this movement. The difference between the two propositions is clearly of importance: one is the con-firmation of a fact, the other is the *à priori* affirmation of a truth in no wise evident.

Simple "good sense" would have demanded that in view of the impotence of the experiments thus far made, new ones should be undertaken and their results awaited. But the authors of the theory of relativity have gone beyond this and developed mathematically the conse-quences of their principle. Let us enumerate some of these:

The first is that the *ether does not exist:* this is the gravest of the conclusions to which the principle of relativity leads; it obliges us to make a fresh start in all the physics of undulatory movements. In the second place it is necessary to renounce all the mechanics of Newton: we can no longer look to this to explain all phenomena; it is, on the contrary, magnetic and electrical phenomena which must constitute the fundamental principles of the general mechanics of matter.

Absolute and general time does not exist. There is *a local time* in each system in motion, which is the *true* time of the system under consideration. In the general sense of the word *simultaneity does not exist.*

Two events may appear to be simultaneous to an observer located in the system A, while they may seem to take place at different times to an observer located in A'. *The same phenomenon can take place sooner in one of the systems than in the other*

Thus the fundamental idea of *time* is overthrown. It is the same with that of *space*. In the theory of relativity the idea of space, taken alone, retains no mean-ing. It is only *the ensemble of space and time which possesses reality.*

The dimensions of bodies (length and volume) *are no longer invariable.* A rod in repose in a system A will always appear to be shortened when its length is meas-ured in another system A' which is in motion with respect to the first. A sphere in repose in one of the systems therefore presents itself to an observer located in the other as a flattened ellipsoid.

Energy must be, still according to the ideas of relativity, *possessed of inertia*. It is analogous to matter. That which we call the mass of ponderable matter can trans-form itself into the mass of energy, and *vice versa*. More-over, energy must have an existence of its own, inde-pendent of all material support; it must possess the power of being emitted or absorbed by bodies and of propagat-ing itself in space, which is strictly empty. In short, energy must possess an atomic structure.

A final consequence of the principle of relativity is that *it is impossible for any relative speed whatever to be superior to the speed of light*. The latter, therefore, must be a *critical* speed, and its value a limited value, not capable of being surpassed. This consequence would be serious, for, at least in imagination, *we can conceive of* a speed greater than that of light.

It is of use to add to the enumeration of these para-doxical pronouncements the new mathematical form given by Minkowski, in 1908, to the principle of rela-tivity.

Starting from the theory that the ideas of space and of time considered separately must be abandoned, and that only their reunion possesses an individuality, this savant reunites them in an indivisible entity which he calls the *universe*. This universe is expressed, in mathe-matical language by *a space of four dimensions*, in which time plays the *rôle* of the fourth dimension. Thus a point in the "universe," as conceived by Minkowski, will have three coördinates: x, y, z, and a fourth, which we may suppose equal to ct.

Such are the principal consequences to which the principle of relativity leads, by mathematical deductions which it would take too long to reproduce here, and which it suffices to indicate.

If these consequences were accepted they would completely overthrow science by forcing us to change its foundations. And this overthrow, without precedent in its history, would be much more considerable than that which was formerly caused by the idea of the motion of the earth around the sun which succeeded the ancient concept of the earth as the center of the universe.

It is but just to add, that if certain savants regard this theory as a definite conquest of the human mind, firmly established in science, there are numerous others, and no mean ones, who look upon it as a philosophic subtlety and entirely reject it, basing this upon the inevitable necessity it involves of absolutely denying the existence of the ether.

As we have already said, it is German scientists who have been the ardent apostles of this new idea. And they seem to have forgotten, herein, that "good sense" is the necessary base of all science.

*From *Larousse Mensuel*.

Poisoning by Primroses

The extensive mobilization of recruits for war now under way in this country will involve the introduction of many thousand city-bred youths into rural surround-ings while undergoing training. It may be well for those thus unaccustomed to country vegetation that not all of it is as innocent as its beauty suggests. Many prim-roses, for example, are capable of causing serious cases of dermatitis, which have even been known in certain instances to result in death.

The *primula Sinensis* and the *primula obconica* are especially toxic. While it is naturally gardeners and horticulturists who are most exposed to these affections, others sometimes suffer merely from plucking a nosegay of the blossoms.

The symptoms, says *Larousse Mensuel* (Paris), ordinarily resemble those of erysipelas. The malady begins with a slight redness of the skin and later small vesicles form an eruption. These unite of themselves to form larger ones, filled with a serous liquid of yellowish tint. These are found mainly on the face and neck, the hands and wrists, and generally exclusively on the uncovered portions of the body. The symptoms are generally brief in duration and not very intense. How-ever a case should be cited where death resulted by secondary infection perhaps. In this instance the affection first attacked the nose, which showed an intense inflammation resembling that of anthrax, so severe that amputation of the tip was necessary. This operation, however, did not arrest the progress of the disease. A general edema of the face followed and the invalid finally succumbed to pneumonia, though it could not be de-termined whether this resulted from a general poisoning of the system or was a secondary affection, which the debilitated subject was not able to throw off.

These dermatites, which sometimes resemble eczema, are caused by the contact with the skin of the tricellular hairs with which the underside of the primrose leaves is covered. The last of these three cells secretes a product which on being evaporated gives crystals of a brilliant yellow. The characteristic dermatitis can be produced by the action of the crystals thus isolated. Prompt diagnosis is very important, so that the source of the trouble may be cut off. The treatment is simple, such as applied to similar eruptions. There are various other plants which seek similarly to protect themselves from enemies, including the *rhus toxicodendron*, the euphor-bias, the hellebores, thapsia, chelidoine, and certain *renonulacea*.

Wayside Crosses in England*

A VENERABLE book of 1496, discussing wayside crosses, at cross-roads, near villages, says:

"For this reason ben crosses by ye waye, that whan folke passynge see ye croysses they shoulde thynke on Hym that deyed on ye croysse above al thynge."

I am afraid familiarity has rather deadened that old-time pious recollection, and that wayside crosses, ancient and venerable as they are, are more commonly looked upon as interesting antiquities than as reminders of what we owe. For attitudes of mind change; so also do the circumstances of the roads.

I suppose that we have really few crosses so old as those rugged examples of granite seen so plentifully in Cornwall. The reason of their being there in that once wild western land is found in the fact of Cornwall having been a roadless and almost trackless region, far beyond the admirable system of Roman roads which penetrated into almost every other extremity of Britain.

Stringston Cross, Somersetshire.

If you know anything of Britain under the Romans you will know that Roman roads are non-existent westward of Exeter. It is really amazing that those wonderful colonizers, in all their four hundred years and more in this country, never actually exploited the West of England. When I think of it I respect the Romans a little less.

Well, then, without roads Cornwall needed with a peculiar urgency some sort of indication by which travelers could find their way across the lonely moors, and thus the characteristic Cornish crosses came into existence.

They date in many instances from the seventh, eighth, and ninth centuries, and in some cases, no doubt, owe their origin to the Irish missionary saints who established their oratories and baptistries in those wilds. A typical example is that of Madron, north of Penzance.

In Cornwall the superstition of treasure being buried beneath these crosses and the frequent inscribed stones has led from time to time to their being dug up, to see what might possibly be underneath.

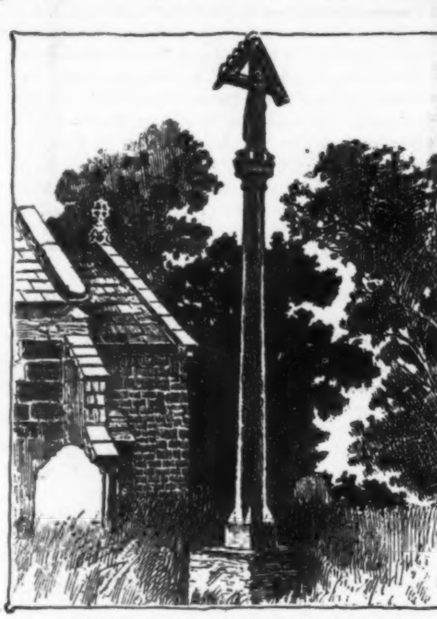
Even the classes who ought to have known better were affected by this fantastic notion. Carew, the quaint old historian of Cornwall, tells of such an incident at Castledour, near St. Austell:

"Not many years sithence," says he, "a Gentleman, dwelling not farre off, was perswaded by some information or imagination that treasure lay hidden under this stone; wherefore in a faire Moon-shine night, thither with certaine good fellowes hee hyeth to dig it up; a working they fall, their labour shortneth, their hope increaseth, a pot of Gold is the least of their expectations. But see the chance. In midst of their toyling, the skie gathereth clouds, the Moone-light is overcast with darkenesse, doune falls a mightie showre, up riseth a blustering tempest; the thunder cracketh, the lightning flasheth; in conclusion, our money-seekers washed in steade of loden; or loden with water in steade of yellow earth, and, more afraid than hurt, they are forced to abandon their enterprise and seeke shelter of the next house they could get into.

John Kemble's grave and Welsh Newton Cross, Monmouth

"Whether this proceedeth from a naturall accident," continues our historian, endeavoring to draw a moral, "or a working of the devill, I will not attempt to define. It may be, God giveth such power over those who begin a matter upon covetousnesse, to gaine by extraordinarie meanes, and prosecute it with a wrong, to the Prince's hurt."

*The Autocar



Old churchyard cross at Somersby, Lincolnshire.

This is very precious!

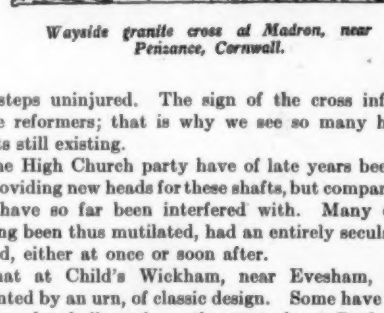
Beautiful indeed are many of the old churchyard crosses of the fourteenth and fifteenth centuries, such as those of Somersby, Tennyson's native village in Lincolnshire, Bitterley in Shropshire, and Stringston, Somersetshire.

These are of the simpler kind, but always with a rich variety of design. The grey, green, and yellow lichens of ages stain and color them and given an added beauty.

. . . . . .

Very many of the old crosses have been secularized, so to speak—or paganized, if you like it better—by the abolition of the cross which once surmounted the shaft.

Nothing was easier for the Puritans of the seventeenth century to do than to knock the heads off the crosses in towns, villages, or by the wayside, leaving the shaft and the steps uninjured. The sign of the cross infuriated those reformers; that is why we see so many headless shafts still existing.

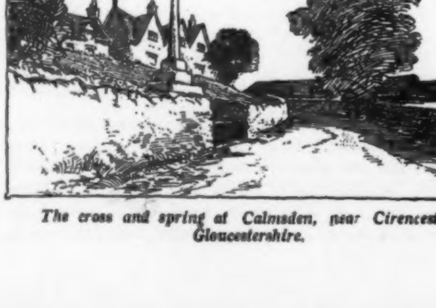


Wayside granite cross at Madron, near Penzance, Cornwall.

The High Church party have of late years been busy in providing new heads for these shafts, but comparatively few have so far been interfered with. Many crosses, having been thus mutilated, had an entirely secular head added, either at once or soon after.

That at Child's Wickham, near Evesham, is surmounted by an urn, of classic design. Some have a stone cube and a ball, such as the example at Backwell, in Somerset, or a plain cube, like that cresting the fourteenth century shaft at the hamlet of Calmsden, near Cirencester.

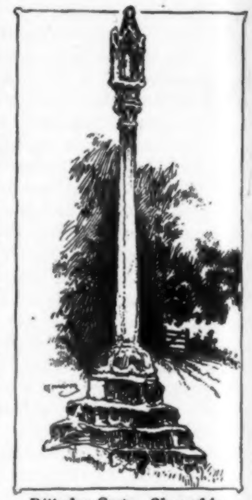
That is a curiously situated cross at Calmsden, presiding as it does over a copious and perennial spring of ice-cold water. Calmsden is, as its name would seem to imply, a home of perfect quiet; but, as a matter of fact, the place-name has no such meaning. It was originally, as appears from a ninth-century Saxon charter, "Kalemund'sdene," that is to say, the vale belonging to a landowner of that name.



The cross and spring at Calmsden, near Cirencester, Gloucestershire.

The stone cube replacing the original cross was no doubt intended to serve the useful office of a sundial, as we see at East Hagbourne, Berkshire, but the work has never been completed. The stonemason performed his part, but apparently Calmsden never could command the visit of an expert to calculate the latitude, longitude, mean time, and difference of equation necessary, and then to set the gnomon and engrave the dial correctly.

Very frugal indeed are the uses which such old crosses have been made to serve. Sometimes they are crested with weather vanes, and that at Stow-on-the-Wold carries a lamp.



Bitterley Cross, Shropshire.

At Braithwell, near Rotherham, Yorkshire, there are the poor remains of an ancient cross, carefully railed round, now that it is scarce possible to do it any further harm. It bears an inscription setting forth that it was set up in the reign of Richard I, to commemorate the liberality of the inhabitants in contributing towards the ransom demanded by the Austrians for his release from captivity.

Among crosses removed altogether from their original sites, I would like to mention that of Cirencester. It was known of old as "Cirencester High Cross," and it stood in the Market Place of that town until 1785. Those were the times when fanaticism had ceased to be, and when indifferentism had taken its place.

The eighteenth century is one for which I nurse an especial distaste! It was an era of paganism of prigs, and of an intolerable swank.

Also it was a period of collectors, who considered themselves to be men of taste. The nobility and gentry who had got what they called "taste" generally got it violently, as one would get measles or scarlet fever, and when they were like that they were terrible fellows indeed, ravishing away all kinds of antiquities from their natural settings in order to embellish their mansions or their parks.

And the mayors and town councils, who did not pretend to taste and really hated things venerable and historic, were, as a rule, only too pleased to oblige any powerful neighboring peer or squire, and gave away any old thing —and mighty glad to be rid of it too! That is how Lord Bathurst in 1785 acquired Cirencester High Cross, and set it up amid the woodlands of Oakley Park.

"Cirencester High Cross," now standing in Oakley Park.

The churchyard cross of Welch Newton, Monmouthshire, was restored some years ago for a very special reason. Beside its time-worn steps there may be seen an old tomb simply inscribed "IK. Dyed the 22 of August 1679." This covers the remains of the man in whose memory the cross was renewed, John Kemble, a Roman Catholic priest who was hanged at Hereford.

He was an old man, very old, and he died with the consolations that extreme age and a philosophical temperament can give. The only request he made before they turned him off was that, being a smoker, he might be allowed a last pipe. This was granted, and he smoked his tobacco comfortably enough on the way to the place of execution.

Coming to the spot he prayed, commended his soul

God, and drew the cap over his eyes. Then the cart was drawn away and he was left hanging. He hanged half-an-hour at least before he died; "yet," said the spectators curiously, "we have never seen anyone die so like a gentleman and a Christian."

They were connoisseurs, those onlookers, of the way in which a man should die. They had had their opportunities of seeing many an one ushered thus violently, and with exceeding ignominy, to that bourne whence no traveler returns.

After all, it is easier to die well than to live well.

Instruments for Testing Curvature of Optical Lenses*

By L. C. Martin, D.I.C., A.R.C.Sc., B.Sc., Imperial College of Science and Technology

For many purposes in the optical workshop it may be desirable quickly to test the curvature of a surface, apart from any very exact determination. There is a very useful little instrument adapted for small, heavily curved lenses, consisting of a brass ring and a central plunger carrying an ivory point. This gives a measure of small radii on a scale, but fails for values greater than an inch or two. The simple instrument here described may be found of use in this connection. It is useful for convex surfaces only, but concave surfaces can easily be tested roughly by other means, such as adjusting a pair of cross wires, brightly illuminated and examined by an eyepiece, to coincide with their own image at the center of curvature. Such an instrument could easily be made to read radii directly if a suitable scale were provided.

Virtual images such as are given by convex surfaces are hard to locate, but the path of a single ray may easily be found. The theory of the suggested apparatus is indicated by the diagram, Fig. 1.

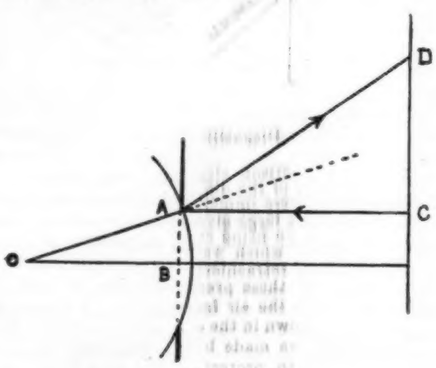


Fig. 1

A ray CA parallel to the axis BO of the curved surface is reflected to a point D on a line CD at right angles to the line CA.

We have if $\angle AOB = \theta$,
$\angle DAC = 2\theta$.

Then $$\frac{DC}{AC} = \tan 2\theta$$
$$\frac{AB}{AO} = \sin \theta$$

And radius of curvature, $R = \dfrac{AB}{\sin \theta}$

Where AB is the distance from A to the axis.

To calculate the value of R for given values of the other constants $\tan 2\theta$ might be expressed in terms of $\sin \theta$ to obtain an algebraical relation. It is easier to work out the value of $\tan 2\theta$ and look up the value of θ and $\sin \theta$ from Chambers' tables, whence R can be obtained. Similarly we can obtain a scale for R along CD In the actual instrument AB is kept constant by putting the lens surface in contact with a fixed circular stop; the diameter of the circle will be equal to 2AB.

A straight slot is cut in a plane piece of wood or metal, and at one end of the slot is placed a pinhole aperture. The top of a strong box of suitable height in which a slot is made serves the purpose extremely well, Fig. 2.

The circular stop is fixed immediately below, so that the edge of the circle is on the line from the pinhole aperture perpendicular to the top plane. A diameter of the stop must be parallel to the slot, and the plane of the circle must be parallel to the top plane. In these previsions we are simply imitating the geometrical condition of the diagram. The perpendicular ray from the pinhole will now always on reflection pass through the slot at a distance varying with the radius of curvature in contact with the stop.

The construction and fixing of this stop are the most important things in making the instrument. The circular hole must be turned out in steel and the top edge bevelled off; the under surface should naturally be planed before the turning is done. The diameter of this stop must be measured accurately by a reading microscope. The diaphragm is then soldered to a mount of stout brass and the whole fixed to a small wooden base ready to be placed in position underneath the slot. A small piece of plane glass is pressed against the diaphragm, and the underside of the slot is brightly illuminated by an electric lamp. On looking through the slot its image in the piece of glass is seen, and this image must be made to cross a marked diameter of the diaphragm. The remaining adjustment is to ensure that this diameter is parallel to the slot itself. A pinhole made in tinfoil is stuck to the undersurface of the slot, so that a line from it at angles to the slot passes through the end of the marked diameter. This position for the pinhole may be found by measurement. The *image* of the pinhole seen through itself must be made to come to the same point at the edge of the circle. These necessary adjustments can easily be made by moving and wedging or cutting the wooden mount.

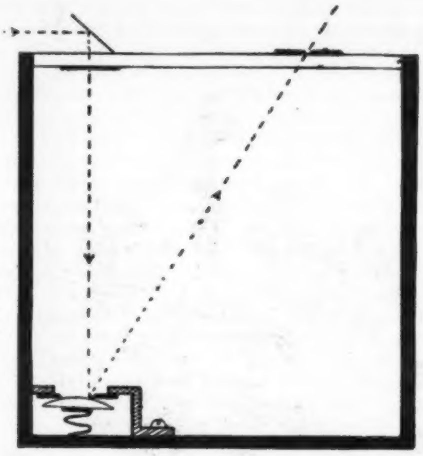


Fig. 2

A small brass plate slides against a rest so that a small hole in it moves over the slot. It is best to drill a hole about 2 mm. in diameter in the brass, and underneath this to fasten a smaller pinhole in tinfoil. The instrument is now ready for the scale, which may be calculated out as indicated. When placed in position a fiducial mark on the movable indicator should indicate ∞ on the scale when the pinholes are superposed.

To obtain the radius of curvature of a surface it is lightly held against the diaphragm by a spring and the fixed pinhole illuminated from above. The bright image in the surface is viewed through the indicator, which is moved away till the image is just hidden by the edge of the stop. The fiducial mark gives the radius of curvature.

The necessary dimensions of the instrument depend on the type of curves for which it is required. For small lenses having radii from 3 to 10 cms. convenient dimensions will be:

Diam. of stop 1.5 to 2.0 cms.
Distance AC 25 to 30 cms.

For small telescope objectives, etc., with shallower curves a bigger instrument must be used,

Diam. of stop = 3 cms.
Distance AC = 100 cms.

PENDULUM CURVATURE APPARATUS

The radius of curvature of a convex surface may also easily be found by mounting the surface on a pendulum rod so that the center of oscillation coincides with the center of curvature of the surface. Under these circumstances the vibration of the pendulum will not move the surface out of a certain spherical locus, and the images of objects formed by reflection therein will appear to be stationary.

A very simple method of observing a slight motion of the images is as follows: (See Fig. 3.) Place a straight edged piece of card just above the surface so that the image of a circular electric lamp shade is nearly hidden by the image of the card. A very slight displacement of the images will result in the lengthening of the strip of shade seen.

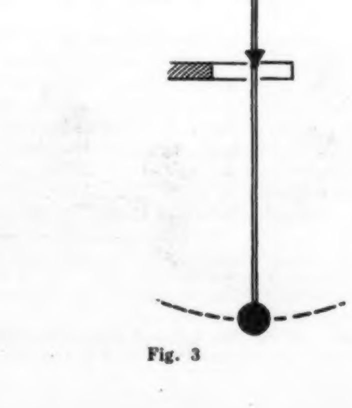


Fig. 3

It is convenient to fix the lens by a little wax to a small disk at the top of the pendulum rod. Place the test surface downwards on a ring on a horizontal table and have some arrangement to hold the length of the rod vertically above the center of the ring. Bring the waxed disk down and pick up the lens.

The rod carries a scale, zero being at the top, and passes through a short metal tube holding a clamping screw; the tube has the knife edges flush with one end.

The length of the pendulum rod above the knife edges is adjusted till the images show no variation, the scale reading given by the knife edges plus the thickness of the lens measures the radius of curvature of the surface. The accuracy is about 1 per cent.

An alternative method of detecting any movement of the image is to view it through a short focus telescope provided with cross-wires. The distance form the knife edges to the apex of the surface may also be found with calipers or a reading microscpoe. Under these circumstances the mean of several results can be taken as correct to about 0.5 per cent.

Since the writing of this paper a somewhat similar method for obtaining refractive indices has been published[1] by Mr. R. W. Cheshire, B.A., based upon the Schlieren-method of Töpler. The adjusted refractive index of a liquid is taken by means of a Pulfrich refractometer, the results obtained being consistent within a few units of the fifth decimal place.

The comparative simplicity of the spectrometer method and its availability for direct measurement with a hydrogen vaccum tube may, however, recommend it in cases where the necessary accuracy can thus be obtained.

*Transactions of the Optical Society (London).

[1]Phil. Mag., October, 1916.

Igorot Method of Spinning Thread

Cotton is spun by means of a sort of plumb bob. This is given a whirling motion by rubbing the spindle-like projection of the bob on the bare thigh. A hollow bamboo length filled with cotton, is so held in the left hand that the thread in twisting allows the plumb bob just to clear the ground. As the bob whirls, the bamboo is slowly raised to lengthen the thread being spun. When this is as long as the worker's reach permits, the thread is wound around the lower part of the bob, is again fastened to the hook in the top of the bob, and the process is repeated. Another method of spinning is by means of a wheel turned by hand while the thread is run between the toes of the spinner, who sits on the ground beside the wheel.—*The Philippine Craftsman.*

Paper-Making Possibilities

Owing to the growing scarcity in Wisconsin of wood suitable for making paper pulp, the Forest Products Laboratory has just completed a study into the methods of barking, chipping, screening and baling of chips. Laboratory tests show that certain western woods are admirably adapted for manufacture into pulp, and negotiations are now under way between paper companies in Wisconsin and western railroads with a view to securing freight rates on trainload shipments of chips to Wisconsin. It is estimated that some of these western woods can be cut into chips, which, when dried and baled, can be delivered to the mills in Wisconsin at a very small advance over the cost of chips made from local timbers. Since there is a market for more than 300,000 cords of wood annually in Wisconsin, an attempt to utilize western species appears worthy of consideration in order to hold the supply of wood for our American paper mills on American soil.—*American Forestry.*

Effect of Corrosion on the Ductility and Strength of Brass*

By Paul D. Merica

Associate Physicist, Bureau of Standards

INVESTIGATION[1] during the last few years has established the fact that brass will crack or fracture when exposed to the action of corroding agents, while at the same time under tensile stress, even when the value of this stress is much less than the ultimate strength or even the yield point of the material, as determined in the tensile test. It is well known that surface corrosion plays an important part, even a necessary one, in the "season cracking" of brass, and Jonson[2] has carried out a number of experiments, in which he has subjected brass test specimens to tensile stress in a testing machine, the specimens being at the same time immersed in a solution of concentrated ammonium hydroxide. He found that so long as the applied stress in these tests was not greater than the elastic limit of the material, the specimens did not fail, but when a stress was applied greater than this limit—*e. g.*, from 20,000 to 40,000 pounds per square inch—cracks would appear after a few days, the specimen would yield continually and finally break.

The effect of corrosion, at least of that caused by the action of ammonium hydroxide, on brass under stress be it initial or due to external load, is to decrease the strength and also the ductility of the material, for it is a striking feature of such brass failures as occur, due to combined stress and corrosion, that they occur with little elongation; the brass does not display its usual ductility. Parallel to his stress-corrosion tests, Jonson ran tensile tests on specimens of the same materials, subjecting them to the same loads, but not at the same time to the action of the ammonium hydroxide; such specimens did not fail after months of test. Furthermore, specimens were corroded with ammonium hydroxide and subsequently tested in tension, giving quite normal results, such that it must be considered that it is the simultaneous effect or action of tensile stress and corrosion (ammonium hydroxide) which produces failure in brass at stresses below its ultimate strength.

It may here be noted that Jonson used only ammonium hydroxide in his tests; he draws the conclusion, however, that any corroding agent, even water, will in time produce the same effect of cracking. The admissibility of this further conclusion seems probable, but is not to be regarded yet as proven.

The author wishes to advance an explanation for this rather striking phenomenon, of the combined action of tensile stress and corrosion on brass, indicating the actual manner or "mechanism" by which this effect is produced, in the hope that this explanation will contribute to progress in dealing with this important technologic problem.

The initial effect of corrosion on any metal, stressed or unstressed, is to roughen the surface, such that in section it appears with ridges or furrows. This nonuniform action of the corroding agent is due to slight variations of the electrolytic (solution) potential over the original (smooth) surface; in the case of a nonhomogeneous alloy such as an alpha-beta brass—type, 60 per cent copper and 40 per cent zinc—a difference of potential would in general exist between the alpha and the beta constituents. If the corroded bar were under tensile stress, the value of the fiber stress at the surface would, theoretically at least for a very long bar, be the same at all points; this can no longer be true, however, of the roughened surface.

Leon[3] has shown that the stress at the bottom of a semicircular notch, in a tensionally stressed bar, is twice that of the average stress for the section, and that for sharper notches the stress, at least for hard rubber, might be as much as five times the average stress. The stress at the free edges of the notch is under such circumstances practically zero. The fiber stress along the roughened surface will therefore vary in value between zero, at the top of the small ridges, and a value, at the bottom of the furrows, much larger than that of the average stress.

Now the electrolytic potential of a metal or alloy is increased[4]—*i. e.*, made more electropositive—by the application of a stress. The results of some measurements and further discussion of this effect of stress on the solution potential will be given below, under the next heading.

It is therefore evident that after the formation of the furrowed or roughened surface on the brass specimen under tension, the emf will be greater at the bottom of the small nicks or notches than on the side of these notches immediately adjacent. Corrosion therefore, other things being equal, will be more severe at the bottom of these notches than elsewhere, and the notches will grow inward, becoming sharper and sharper, quite in contrast to their behavior when uncorroded, since in a ductile material such notches tend to flatten or smooth out under tension, thus reducing the local fiber stresses at these points.

In fact, Heyn[5] finds that the behavior of a notch in a bar under tensional stress distinguishes brittle from ductile material. The notch in a ductile material tends to smooth out, whereas in the brittle material it becomes sharper and narrower under tension, finally causing fracture. Thus, the effect of corrosion under stress is to favor the growth and narrowing down of cracks and to cause an apparent brittleness of the brass.

MEASUREMENT OF THE ELECTROLYTIC POTENTIAL OF BRASS UNDER STRESS

The increase of electrolytic emf of a simple metal to a solution (containing ions of the same metal) due to the application of a stress may be calculated[6] from the consideration that the increase of emf of a system is proportional to the increase of free energy, and that for isothermal, reversible processes the increase of free energy of a system is equal to the amount of work done on it. The application of stress of values below the true elastic limit is a reversible process and can be carried out isothermally. Considering, for the purpose of simple and approximate calculation, that brass is a simple metal of equivalent weight equal to

$$\frac{63+65}{2\times 2}=32,$$

a stress of 25,000 pounds per square inch, applied isothermally to a brass rod, having a modulus of elasticity of 15×10^6 pounds per square inch, and a density of 8, should produce an increase of emf equal to

$$\frac{\dfrac{\text{work (in ergs)}}{\text{density}}\times\dfrac{\text{equivalent weight}\times 10^{-7}}{96{,}500}}{}$$

$$=\frac{\dfrac{25{,}000}{2}\times\dfrac{25{,}000}{15{,}000{,}000}\times\dfrac{1000\times 980}{14.2}\times 32\times 10^{-7}}{96{,}500\times 8}$$

$=0.000006$ volt;

with greater stresses and after plastic yielding had commenced, an increase of 2 millivolts might be obtained, assuming that one-half of the energy of deformation is potentialized.

Some measurements have been made of these quantities, with, however, contradictory results. Hambuechen (loc. cit.) finds for brass—composition of the brass and of the electrolyte not stated—an increase of 0.3 millivolt at the elastic limit, and an increase of about 4 millivolts at the point of rupture. For iron and steel he finds much larger values. Both Walker and Dill and Richards and Behr, on the other hand (loc. cit.), come to the conclusion that, although there is an increase in the emf of iron (they did not work with other metals) caused by application of stress up to and beyond the elastic limit, it is so small as to defy measurement; they explain Hambuechen's results on iron as having been due to the hydrolysis of the ferric salt, which he used as electrolyte.

In the measurements made by the author use was made of two electrodes of the same material, and in the same condition; one of these could be stressed, the other

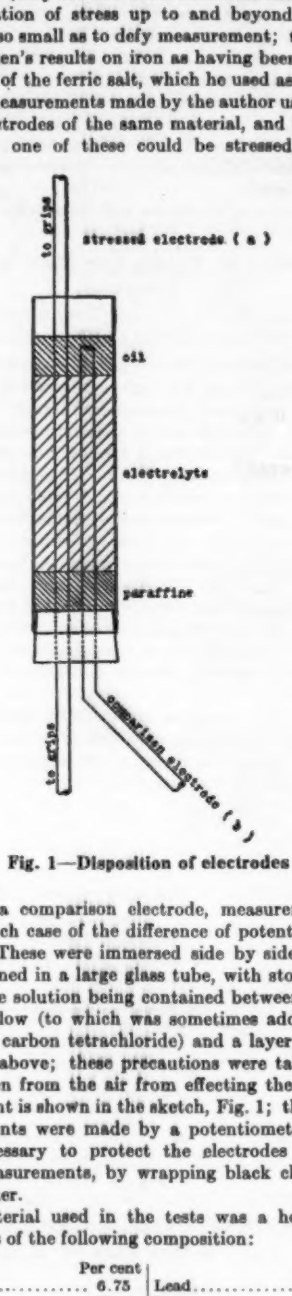


Fig. 1—Disposition of electrodes

served as a comparison electrode, measurement being made in each case of the difference of potential between the two. These were immersed side by side, in a solution contained in a large glass tube, with stopper at the bottom, the solution being contained between a layer of paraffin below (to which was sometimes added a slight amount of carbon tetrachloride) and a layer of a transformer oil above; these precautions were taken to prevent oxygen from the air from effecting the emf. The arrangement is shown in the sketch, Fig. 1; the potential measurements were made by a potentiometer. It was found necessary to protect the electrodes from light during measurements, by wrapping black cloth around the container.

The material used in the tests was a homogeneous alpha brass of the following composition:

	Per cent		Per cent
Copper	6.75	Lead	0.06
Zinc	32.5	Iron	0.02
Tin		Manganese	

This material was most kindly furnished by the American Brass Co., in the form of one-fourth inch rods. Before the tests, this material was annealed at 400° C. for an hour in order to relieve any initial stress, and the surface thereupon prepared by light rubbing with fine emery paper, followed by etching with nitric acid, washing and drying.

The test procedure consisted in placing the specimens in position, with the electrode (*a*) under zero stress, noting from time to time the emf between (*a*) and (*b*), then applying a stress, in units of 10,000 pounds per square inch, again noting during a certain period of time the emf, removing the stress, noting the emf, reapplying the stress, etc. The results of these measurements are best studied in the form of a curve, giving the emf as a function of the time, a change of stress being noted on the curve. Two such curves showing typical results obtained are given in the Figs. 2 and 3.

It is seen that there is a small but unmistakable increase in the emf, due to the application of stress, and

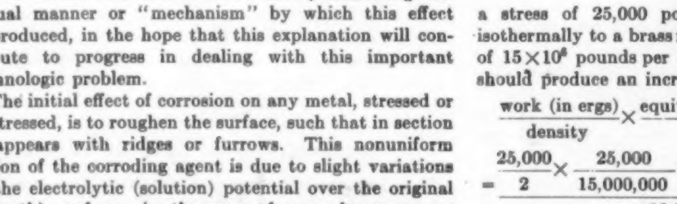


Fig. 2—The emf of stressed to unstressed brass to a solution of N/10 Cu SO4 Zn SO4. The + sign indicates that the bar under stress is electropositive to the comparison electrode.

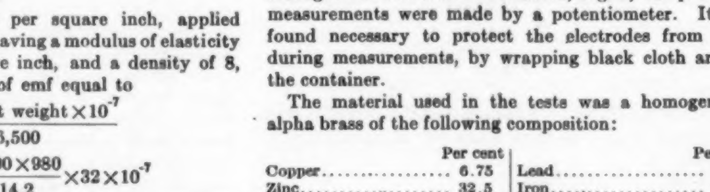


Fig. 3—The emf of stressed to unstressed brass in a solution of NZn SO4. The + indicates that the stressed bar is electropositive to the comparison electrode

*Technological Paper No. 83, of the Bureau of Standards.

[1] E. Heyn, Internal Stresses in Cold-Wrought Bars, and Some Troubles Caused Thereby, J. Inst. Metals, 12, 1914, p. 3; A. D. Flinn, Brass in Engineering Construction, Engineering Record, 68, 1913, p. 527; P. D. Merica and R. W. Woodward, The Failure of Structural Brass, Trans. Amer. Inst. Metals, 1915

[2] E. Jonson, Failures of Forgible Brass Bars, Trans. Amer. Inst. Metals, 8, 1914, p. 135; The Fatigue of Copper Alloys, Proc. Amer. Soc. Test. Materials, 40, 1915, p. 101.

[3] Leon, Uber die Spannungsverteilung in einer halbkreisformigen Kerbe, Osterreichische Wochenschrift fur den offentlichen Baudienst, 29, p. 43; 1908.

[4] C. Hambuechen, The Corrosion of Iron, Bull. Univ. Wisconsin, No. 8, 1900; T. Richards and Behr, The EMF of Iron Under Various Conditions, Publ. Carnegie Inst., No. 61, 1906.

[5] E. Heyn, Materialienkunde, p. 376.

[6] Walker and Dill, the Effect of Stress on the EMF of Soft Iron, Trans. Amer. Electrochem. Soc., 7, p. 153; 1907.

that this becomes relatively quite large, in Fig. 2, when the yield point of the brass is reached.

It is now to be noted that when a bar of metal is subjected to a tensile stress a lowering of temperature takes place, and this lowering of temperature itself produces a momentary change in emf, which often masks the permanent one; this effect can be noticed in the curves, and it is for this reason that the author adopted the method of holding the specimen at a constant stress for some time, and noting the emf, instead of using Hambuechen's method of continually increasing the stress during the course of the measurements.

CONCLUSIONS

It is believed that an increase in emf of alpha brass to solutions containing zinc and copper ions, caused by the application of tensile stress, has been indicated and measured. This amounts to about 0.2 millivolt for 20,000 pounds per square inch (below the elastic limit) and to about 1 millivolt at the yield point of the material, 30,000 pounds per square inch. This value is apparently much greater than would be calculated for elastic stresses from thermodynamic considerations; it agrees well with values calculated for stresses above the elastic limit. The author does not wish at present to discuss this divergence otherwise than to point out the emf measured may be that at some point of the surface, *e. g.*, at the bottom of a small notch, where the fiber stress is much greater than the average; in that case these results could be brought into agreement with thermodynamic theory.

An explanation is given of the effect of corrosion on brass under stress, in decreasing the ductility and strength, and which is based upon this fact of the increased emf and therefore corrodibility of brass under stress. At the bottom of small furrows in the roughened surface the stress is greater than at ridges immediately adjacent; a galvanic couple is formed, and the bottom of the furrow only is corroded, forming in time a crack, which becomes narrower and sharper as it penetrates, finally so reducing the cross section that fracture occurs.

This conception of the cause of the embrittling effect of corroding agents on brass under stress is borne out by the examination of samples which have been subjected to Jonson's test. In such samples, not one but several fissures appear, narrowing down and becoming microscopically fine as they penetrate farther and farther into the interior of the specimen. It may be noted in this connection that these fissures appear to favor the beta constituent and to avoid the alpha in a brass, such as manganese bronze, containing both of these.

The question as to why brass should be so susceptible to this effect, and not also iron and steel (apparently, at least), can not at present be adequately answered, not indeed until further investigation has considerably increased our knowledge concerning the values of the stress-emf effect in these materials, the relative significance of this effect in comparison with other local emf's (due to oxide and slag inclusions, etc.), the effect of the covering of corrosion product formed, and other points.

Apparently the effect produced by corroding solution having a low conductivity will, other things being equal, be greater than that of one of high conductivity, since it allows greater prominence to those local stress emf's between portions of the material immediately adjacent. It is known, for example, that nitric acid does not produce cracking in initially overstressed brass in the manner in which ammonium hydroxide or mercurous nitrate or chloride does.

Active Beavers at the New York Zoo

For the past year there has been a marked difference of opinion between the men in the Forestry Department and our beavers. When we constructed the beaver enclosure and regulated the drainage of the pond, a pipe was placed near the top of the dam, where the animals so industriously work. The object of this pipe was to form a sort of spillway for the dam-work to be designed and executed by the beavers. To prevent the pond from overflowing its banks, it is quite necessary to keep the pipe open. That necessity is constantly combated by the beavers, which persistently stuff the pipe full of twigs, leaves and mud to prevent it from performing its function. As it takes several days for the beaver to completely block the pipe, it is cleaned out at intervals a few days apart.

The night after the drain has been cleared is a busy one for the beavers. In their first rush to combat the change, and stop a wicked waste of perfectly good water, they use every kind of *debris* available. Even corn husks are not scorned in the reconstruction work.—*Bulletin of the New York Zoological Society.*

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